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THE TOTAL SOLAR ECLIPSE OF JULY 29TH, 1878.

By Prof. HENRY DRAPER, M. D.

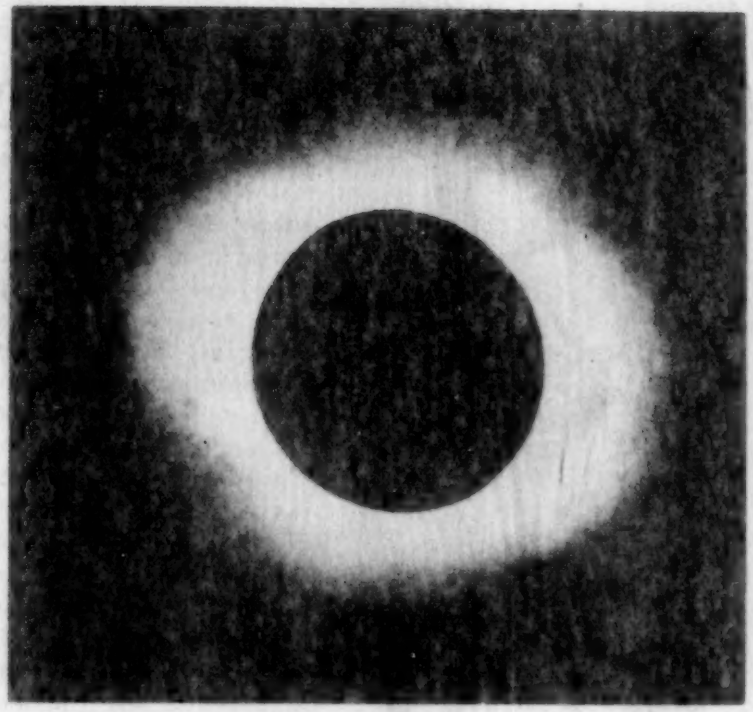
As the eclipse of July 29th occurred at a time when there were no sun spots and the chromosphere was in a condition of minimum activity, it seemed probable that new facts concerning the corona might be ascertained. For this reason I organized an expedition, consisting of my wife and myself, my friends, Profs. Morton and Barker, and Mr. Edison. Rawlins, Wyoming Territory, was selected as the station for a temporary observatory, because, while it was near the central line of totality, it was supplied with water from the granite of Cherokee Mountain, which facilitated photographic operations, and was in a region that was dry and apt to be cloudless. Being on the line of the Union Pacific R. R., apparatus could be easily transported.

The instruments we took with us weighed about a ton, and consisted, 1st, of a telescope of 5 inches aperture and 78 inches focal length, especially corrected for photography by Alvan Clark & Sons.

This enabled me to get as well defined a photograph of the corona as could be obtained by an exposure lasting during the whole of totality, that is, 165 seconds. 2d, a large instrument which may be called a phototelespectroscope, Fig. 1. It consisted of an object-glass composed of four lenses, 6 inches in diameter and 21 inches focal length, loaned to me by Messrs. E. & H. T. Anthony of New York. The image of the sun at the focus of this instrument was less than $\frac{1}{4}$ of an inch in diameter, and of extreme brilliancy. Before the rays of the lens reached a focus they were intercepted by a Rutherford grating about 2 inches square, set at an angle of 60 degrees. This threw the rays to one side and produced three images, a central one of the sun and on either side of it a spectrum. One of these spectra was dispersed twice as much as the other, that is, would give a photograph of twice the length. This instrument was mounted equatorially, as was also the five inch telescope. They were driven by clockwork to follow the sun with accuracy. For this equatorial mounting and driving clock I am indebted to Prof. Pickering, Director of Harvard Observatory. 3d, Prof. Barker brought from the collection of the University of Pennsylvania, 2 four-inch achromatic telescopes, one with a Merz direct vision spectroscope attached to it for eye observations, the other to be used with Edison's tasimeter. Besides these there were polariscopes, a grating spectroscope, an eye slitless spectroscope with two-inch telescope, and a full set of chemicals for Anthony's Lightning Collodion Process, which in my experience is three-times quicker than any other.

Fig. 1.

Our plan of operations was as follows: 1st, the photographic and photo-spectroscopic work, as well as the eye slitless spectroscope,



HELIOGRAPH. J. CARBUTT. PHILA.

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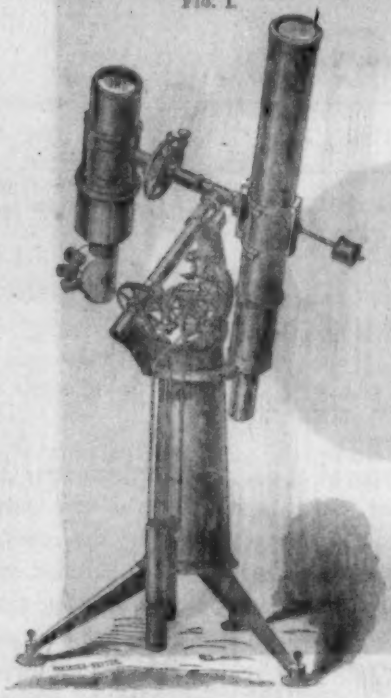

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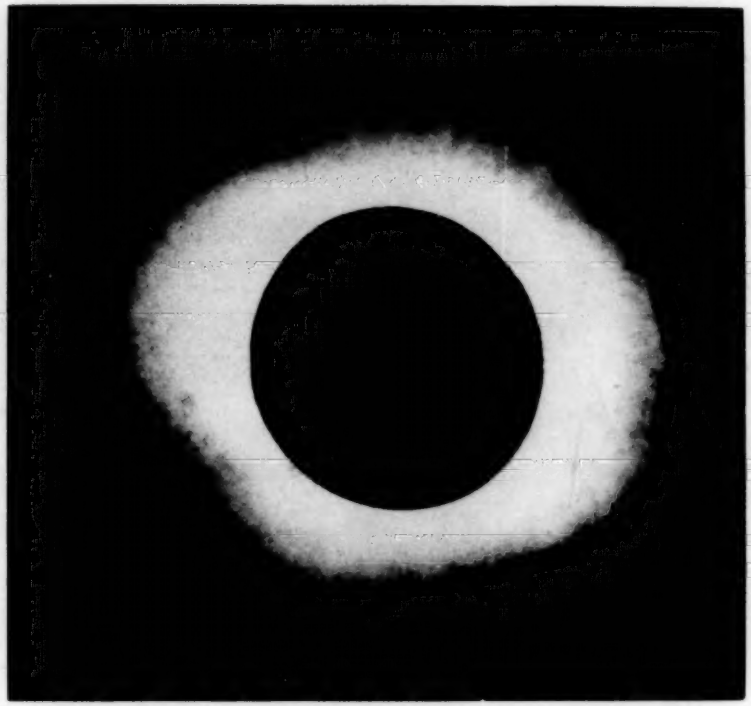

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were to be in charge of my wife and myself. 2d, the analysing slit spectroscope was in charge of Prof. Barker, with the especial object of ascertaining the presence of bright lines or else of dark Fraunhofer lines in the corona. 3d, the polariscopic examinations were confided to Prof. Morton, who was also to spend a few moments in looking for bright or dark lines with a hand spectroscope. 4th, Mr. Edison carried with him one of his newly invented tasimeters with the batteries, resistance coils, Thomson's galvanometer, etc., required to determine whether the heat of the corona could be measured.

This programme was successfully carried out, and entirely satisfactory results were obtained.

If the light of the corona was glowing gas giving bright lines which lay in the actinic region of the spectrum, the photograph would show ring-formed images, one ring for each bright line, while if the light of the corona arose from incandescent solid or liquid bodies, or was reflected light from the sun, I should obtain a long band answering to the actinic region of the spectrum. If the light was partly from gas and partly reflected sunlight, a result partly of rings and partly a band would be obtained. It was the general impression that it would be impossible to obtain a photograph of the diffraction spectrum of the corona, but the result proved the contrary, for on developing the photograph immediately after totality, it appeared that I had obtained a spectrum without the least trace of a ring, showing that the corona on this occasion did not consist of an incandescent gas giving a few bright lines, but that its light was due to reflection of sunlight by solid or liquid bodies surrounding the sun like a cloud of meteors.

The plain photograph of the corona (see plate), shows that the great mass of the matter lies in the plane of the ecliptic, but not equally distributed, for on this occasion it extended about a degree and a half from the sun towards the west, while it was scarcely a degree in length towards the east. The photograph also shows curious ray-like forms of coronal matter towards the poles of the sun.

There can be little doubt that during this eclipse we have observed the true nature of the corona, and that its light is almost altogether sunlight reflected from bodies of a size too small to be distinguished as individuals. According to this view, the light of the corona has, on former occasions, been infiltrated with materials thrown up from the chromosphere, notably with the 1474 matter and hydrogen. The

1474 line can be seen on any occasion favorable for observations in the chromosphere of the sun. As the chromosphere is now in a quiescent condition, this 1474 matter had not been projected into the corona as in former eclipses, and hence, probably, the pearly white color on this occasion.

The observations of both Profs. Barker and Morton confirmed this theory, that the corona shone by reflected sunlight at this time, for Prof. Barker with the analysing spectroscope observed the dark Fraunhofer lines in the corona, and the polarization was shown by Prof. Morton to be such as would answer to reflected solar light. Prof. Morton also observed the dark Fraunhofer lines in the coronal light with a hand spectroscope.

Mr. Edison used his newly invented tasimeter to ascertain whether the corona gave indications of heat. He adjusted the instrument to be very sensitive and found eventually that a much less delicate adjustment would have sufficed, for the moment the tasimeter was moved from the image of the dark moon to the image of the corona, the index beam of light went entirely off the scale of the galvanometer.

ECONOMIC VAPORIZATION OF WATER AND EXPANSION OF STEAM.

Analysis of Mr. Lavington E. Fletcher's experiments at Dalkeith, Scotland, on the engine and boilers of the paper mill belonging to the Messrs. Craig.

By Chief Engineer ISHERWOOD, U. S. Navy.

[Continued from Vol. lxxvi, page 156.]

The steam in Regnault's well-known experiments on that substance, was generated and condensed under the *same pressure*, and its total heat above the temperature of 32° Fahrenheit, as given in his table, was measured by the quantity of heat imparted to water at the temperature of 32° Fahrenheit, by its condensation. This total heat may, for the present inquiry, be considered as composed of two quantities, namely, the quantity required to vaporize the water, and the thermal equivalent of the work done by the resulting steam in evaporating against the boiler pressure. Of these two quantities,

only the former exists in the steam as heat; the latter appears as heat only when the steam is condensed, but not before—not while the steam exists as such. When steam is both evaporated and condensed under the same pressure, its condensing-water contains the total heat given by Regnault, because the heat transmuted into work during the evaporation of the steam under pressure is exactly regained by the work done by the same pressure upon the condensing steam during its condensation.

In the evaporation of steam against pressure, a measurable amount of work is done which admits of easy calculation, and its thermal equivalent being deducted from Regnault's total heat, gives the quantity of heat effecting the vaporization alone of the water; consequently, the mass of steam which enters the cylinder of a steam-engine, contains only the latter quantity of heat, the former never having been in the steam, because, though abstracted from the furnace, it was transmuted into the work of evaporation.

If steam were worked through the cylinder without expansion, or without being cut off, and exhausted into a condenser in which a perfect vacuum or no pressure existed, then the quantity of heat found in the condenser, as measured by the mass of condensing- or injection-water of a given temperature required to condense that steam into water of any given greater temperature, would be exactly equal to the heat in the steam as it entered the cylinder, in which case the total heat of steam would be less than given in Regnault's table by the thermal equivalent of the work of evaporation.

But if the steam were worked through the cylinder expansively, or cut off, the other conditions remaining as just described, then the quantity of heat found in the condenser would be less than the quantity in the steam when it entered the cylinder, by exactly the thermal equivalent of the work done by the expanded steam, or after the closing of the cut-off valve, which thermal equivalent is abstracted from the heat in the expanded steam.

The effect of this abstraction of heat from the mass of steam in the cylinder, after its isolation from the boiler by the closing of the cut-off valve, is to condense to water, of the temperature of the remaining steam, the necessary quantity to furnish the heat transmuted into the work done by the expanded steam. The weight of this water of condensation, due to such work, can be exactly calculated: the water itself remains intimately mingled, as fine spray, with the

expanded steam, and is swept with the latter into the condenser, where it is cooled by the injection-water, from the temperature of the steam in the cylinder at the end of the stroke of the piston down to the temperature of the condenser.

If steam were a perfect gas, obeying the laws of such an ideal substance, a given mass always occupying bulks in the inverse ratio of its pressure, and never losing the gaseous form by the abstraction of heat, then the work done upon the condensing-steam in the condenser by the pressure there, would exactly equal the work of evaporation done in the boiler, less the work done by the expanded steam in the cylinder; in which case, the calculation of the quantity of heat to be found in the condenser would only require multiplying the number of pounds of steam entering the cylinder, by Regnault's total heat above the temperature of the condenser, and subtracting the thermal equivalent of the work done by the expanded steam. But as steam, instead of being a perfect gas, is a very sensitive vapor, such a process would give only an approximate result.

The quantities on lines 56 to 62, both inclusive, have been calculated according to the above considerations.

Line 56 contains the number of Fahrenheit units of heat above the temperature of the hot-well, in the steam which entered the cylinder during the experiments. These quantities are the products of those on line 10, by Regnault's total heat of steam above the hot-well temperature, less the thermal equivalent of the work of evaporating the steam.

The thermal equivalent, H, in Fahrenheit units, of the work of evaporating one pound of water under the pressure, in pounds per square inch, p, is obtained from the formula, $H = \frac{V \times p \times 144}{789\frac{1}{4}}$: in which V is the number of cubic feet occupied by one pound of steam of the pressure p, 144 are the number of square inches in a square foot, and $789\frac{1}{4}$ are the dynamical equivalent in foot pounds of one Fahrenheit unit of heat.

Line 57 contains the number of Fahrenheit units of heat added to the steam in the condenser during its condensation into water of the hot-well temperature, by the work done upon it by the pressure in the condenser.

The weight of steam, as such, entering the condenser is less than the weight of steam (line 10) entering the cylinder by the weight

condensed in the cylinder to furnish the heat transmuted into the number of total horses-power (line 37) developed by the expanded steam. The weight, W, of steam thus condensed in the cylinder, during the experiments, is obtained, in pounds, from the formula,

$$W = \frac{41{\cdot}811847 \times HP \times m}{L},$$

in which $\left(\frac{33000}{789\frac{1}{4}} =\right)$ 41·811847 are the number of Fahrenheit units of heat equivalent to the work of one horse-power, HP is the total number of horses-power (line 37) developed by the expanded steam; m is the time, in minutes, during which the experiment or development of power lasted; and L is the latent heat of steam of the pressure at the end of the stroke of the piston (line 29). Consequently, the pounds weight of steam entering the condenser, during the experiments, are the quantities on line 10, minus W.

The quantities on line 57 are calculated from the following formula:

$$H = W' \times \frac{\left(V' - \frac{1}{v-1}\right) \times p' \times 144}{789\frac{1}{4}},$$

in which H is the units of heat sought; W' is the pounds weight of steam entering the condenser during the experiments; V' is the number of cubic feet occupied by one pound of steam of the pressure p'; v is the relative volume of steam of this pressure, and the water of the hot-well temperature from which it was generated; p' is the pressure in the condenser in pounds per square inch; 144 are the number of square inches in a square foot, and $789\frac{1}{4}$ are the dynamical equivalent of one Fahrenheit unit of heat.

The quantities on line 58 are the number of Fahrenheit units of heat (H) absorbed by the condensing- or injection-water, in reducing the number of pounds of water of condensation due to the production of the power on line 37, from the temperature of the steam at the end of the stroke of the piston (line 29) to the temperature of the hot-well (line 23).

If h be the Fahrenheit units of heat in the water of condensation due to the production of the power by the expanded steam, when this water is at the temperature of the steam at the end of the stroke of the piston, and h' be the Fahrenheit units of heat in the same water

when at the temperature of the hot-well, W being the pounds weight of this water, then $H = W \times (h - h')$.

The quantities on line 59 are the number of Fahrenheit units of heat (H) abstracted from the expanded steam in the cylinder, after the closing of the cut-off valve, by transmutation into the total horses-power (line 37) developed during the experiments by the expanded steam after the closing of the cut-off valve. These quantities are calculated from the following formula:

$$H = 41{\cdot}811847 \times HP \times m.$$

The quantities on line 60 are the remainders of the sum of the quantities on lines 56, 57 and 58, after the subtraction of the quantities on line 59, and show the number of Fahrenheit units of heat which should have been found in the hot-well in the injection- or condensing-water.

Now the quantities on line 60 are evidently too small, owing to causes foreign to the above stated principles of calculation and operating a loss of heat by the water in the hot-well. These causes might be avoided, or their effect ascertained and allowed for, but this was not done in the present experiments. The temperature (line 23) of the water in the hot-well is too low, owing to the loss of heat by external radiation from the exhaust-pipe, air-pump, condenser and hot-well surfaces in contact with the atmosphere; in a word, from all the external surfaces of the pipes and vessels between the cylinder and the point at which the temperature of the hot-well water is taken. The temperature of this water was likewise further reduced, by evaporation from its exposed surface into the air. In a very accurate experiment these surfaces should be protected by a non-conducting covering, and the temperature of the water ascertained in a closed hot-well.

During these experiments there was, of course, a constant evaporation going on into the air from the surface of the water, in both the feed-water tank and the hot-well water tanks, which might have been ascertained and allowed for, but the effect of which was to make the weight of both the feed-water and the hot-well water appear too great; thus, in measure, establishing a compensation when the quantity of heat in the condensing-water, as ascertained by calculation from the weight of feed-water consumed, is compared with the quantity of heat in the condensing-water, as directly measured. It

is, of course, assumed that there is no leakage of water or steam between the feed-water tank and the hot-well water tanks. Any water foamed over, or primed with the steam from the boiler into the cylinder, need not be considered in this investigation, because it will be vaporized in the cylinder and enter the condenser as steam, the heat of its vaporization being obtained from the boiler by means of the metallic surfaces of the cylinder. The only effects of such priming are the lessening of the power developed by the engine, and the increasing, apparently, of the economic vaporization by the boiler.

Line 61 gives the number of Fahrenheit units of heat actually found in the condensing-water. These quantities are calculated by multiplying the difference of the quantities on lines 12 and 10, by the difference in the units of heat in water of the temperatures on lines 22 and 23.

Line 62 shows the difference, due to all causes, between the number of Fahrenheit units of heat that should have been found in the condensing-water, according to calculation (line 60), and which were there, by direct observation (line 61), expressed in per centum of the latter.

In all cases, the difference of the specific heat of water of different temperatures has been allowed for.

REMARKS.

Of the Effect of the Measure of Expansion with which the Steam is used, on the Economic Development of the Power.—Omitting, for the present, the consideration of the economic results of experiment C, and comparing those of experiments D and E, in which there was no steam in the jackets of the cylinder, but in which the steam was expanded, respectively, 2·3876 and 2·5842 times, it will be perceived that the greater measure of expansion was accompanied by a less economic result, the total horse-power in experiment D being obtained for an expenditure of 26474·0151 Fahrenheit units of heat per hour, while in experiment E it was obtained for 28395·3372 units. This very great difference of 6·77 per centum of the greater quantity, for so very small a difference in the measure of expansion, is impossible, and only proves errors of observation. It will be entirely safe to assume there was no difference in the economic results of these two experiments, and that for the average measure of ex-

pansion of $\left(\frac{2{\cdot}3876 + 2{\cdot}5842}{2} = \right)$ 2·4859 times, the total horse-power cost an expenditure of $\left(\frac{26474{\cdot}0151 + 28395{\cdot}3372}{2} = \right)$ 27434·6762 Fahrenheit units of heat per hour.

Comparing the economic results of experiments A and B, in which the steam-jackets of the cylinder were filled with steam of the boiler pressure, and in which the steam was expanded, respectively, 5·710 and 4·448 times, it will be observed that the greater measure of expansion was again accompanied by a less economic result, the total horse-power in experiment A being obtained for an expenditure of 24722·3538 Fahrenheit units of heat per hour; while, in experiment B, it was obtained for 23197·9061 units. This difference of 6·17 per centum of the greater quantity, against the greater measure of expansion, shows errors of observation. It will be quite safe to assume there was no difference in the economic results of these two experiments, and that, for the average measure of expansion of $\left(\frac{5{\cdot}710 + 4{\cdot}448}{2} = \right)$ 5·079 times, the total horse-power cost an expenditure of $\left(\frac{24722{\cdot}3538 + 23197{\cdot}9061}{2} = \right)$ 23960·1300 Fahrenheit units of heat per hour.

Comparing, now, the average economic results of experiments A and B, in which steam of an average initial pressure of $\left(\frac{58{\cdot}97 + 58{\cdot}02}{2} = \right)$ 58·50 pounds per square inch above zero was used, with an average expansion of 5·079 times, in a cylinder whose jackets were filled with steam of the average pressure of 72·19 pounds per square inch above zero, with the average economic results of experiments C and D, in which steam of an average initial pressure of $\left(\frac{41{\cdot}88 + 39{\cdot}78}{2} = \right)$ 40·83 pounds per square inch above zero was used, with an average expansion of 2·4859 times, in a cylinder whose jackets were not filled with steam; experiments A and B exceed experiments D and E, in the economic production of the power,

$\left(\frac{27434{\cdot}6762 - 23960{\cdot}1300 \times 100}{27434{\cdot}6762} = \right)$ 12·665 per centum; $12\frac{2}{3}$

per centum is all the economic gain obtained from using steam of (58·50 — 40·83 =) 17·67 pounds greater initial pressure per square inch above zero, with double the measure of expansion (2·4859 and 5·079 times), in a steam-jacketed cylinder, the temperature in the jackets being 60·84° Fahrenheit greater than that of the mean steam pressure (line 33) in the cylinder. How much of this small gain is due to the greater initial pressure? how much to the greater measure of expansion? and how much to the steam-jacketing? Unfortunately, the data of the experiments do not permit an answer to these interesting questions.

In experiment C, steam of the initial pressure of 41·48 pounds per square inch above zero, was used with an expansion of 1·882 times in a cylinder whose steam-jackets were filled with steam of 47·19 pounds pressure per square inch above zero, under which conditions the total horse-power (line 36) was obtained for an expenditure of 27011·1996 Fahrenheit units of heat per hour, or about 1·55 per centum less than the average (27434·6762 units) of experiments D and E, made without steam in the cylinder-jackets, but with about the same initial pressure expanded 2·4859 times. Now, is this near equality to be attributed to a compensation established in experiment C, between a supposed loss by the less measure of expansion employed, and a supposed gain due to the presence of steam in the cylinder-jackets? Such a view may properly be taken, and the data are wanting in these experiments to sustain or overthrow it.

Experiment C is the most anomalous of the experiments, and its results cannot be reconciled with those of the others. It was greatly shorter than they, and the indicator diagrams taken were proportionally fewer. The writer is of the opinion that the horses-power developed in it are considerably too small. That there is a large error in it somewhere, is apparent from the fact that the condensation in the cylinder, other than due to the development of the power (line 51), is about equal to the mean condensation in experiments D and E, where steam of about the same initial pressure was expanded one-third more, and in a cylinder without steam in its jackets. Such a result is opposed to every other experiment of which the writer has knowledge. Further, the discrepancy (line 62) between the heat that should have been found in the condensing water, and the heat actually found in it, is too great for accuracy. The writer is of opinion that experiment C should be rejected. If it be accepted, it

shows, on the supposition of steam in the cylinder-jackets being non-effective, that between the measures of expansion, 1·882 and 2·5842 times, the economic development of the power was not affected by the measure of expansion with which the steam was used; or, if the presence of steam in the jackets be supposed effective, then it shows that such efficiency about equaled the economic gain due to increasing the measure of expansion from 1·882 to 2·5842 times.

The supposition of the non-effectiveness of steam in the jackets, combined with the equality of the economic production of the power, with measures of expansion ranging from 1·882 to 2·5842 times, does not accord with the fact that when the latter measure of expansion was increased to 4·448 times (experiment B), and to 5·710 times (experiment A), there resulted an economic gain of about one-eighth. Why should there have been so great an increase in the economy of the steam when used with these latter measures of expansion, than when expanded 2·5842 times, while there was no gain for an increase in the measure of expansion from 1·882 to 2·5842 times, and when there was a loss instead of a gain in going from an expansion of 4·448 to 5·710 times? Nature does not proceed *per saltum*.

If the presence of steam in the steam-jackets is admitted to be as effective in these as it has been found to be in all other experiments made with steam-jacketing, then the one-eighth gain in economy found in experiments A and B, over the results in experiments D and E, is due wholly to the presence of steam in the jackets during the former, and not at all to the increased measures of expansion with which the steam was used in them.

But, taking the experiments as they stand, the fact is apparent that, with the same initial pressure in the cylinder, the engine making the same number of double strokes of piston per minute, and developing the same net horses-power, the area of the piston being proportionately larger with the greater measures of expansion, the net or commercially valuable horse-power would be obtained for as small an expenditure of heat per hour when the steam was used with an expansion of 1·882 times, as when it was used with any greater measure of expansion up to 5·710 times.

All the above comparisons have been made for the cost of the total horse-power in Fahrenheit units of heat expended per hour. This is equivalent to a comparison of the cost of the net horse-power,

on the supposition that the same cylinder is employed with the same speed of piston, and the same net pressure upon the piston per square inch, which requires a proportionately higher and higher initial pressure as the measure of expansion is greater and greater.

Relative Economic Efficiencies of the two kinds of Boilers and Coals.—A comparison of the vaporization results in experiments A and E (line 54), shows that with the Sinclair boiler, notwithstanding its large proportion of 35·95 square feet of heating-surface per square foot of grate-surface, an increase in the rate of combustion of Scotch bituminous coal, from 13·2321 to 16·6076 pounds per hour per square foot of grate-surface, decreased its economic vaporization $\left(\frac{8\cdot2337-7\cdot3946\times100}{8\cdot2337}=\right)$ 10·19 per centum.

The Scotch bituminous coal consumed in the Sinclair boiler (experiment A), gave an inferior economic vaporization (line 54) to the Welsh semi-bituminous coal consumed in the same boiler (experiment B) of $\left(\frac{10\cdot0569-8\cdot2337\times100}{10\cdot0569}=\right)$ 18·33 per centum, both coals vaporizing about the same weight of feed-water per hour from the same temperature.

The common Lancashire boiler having the small proportion of 19·08 square feet of heating-surface per square foot of grate-surface, gave, when burning the Scotch bituminous coal at the rate of (mean of experiments C and D, $\frac{22\cdot7808+23\cdot1694}{2}=$) 22·9751 pounds per hour per square foot of grate-surface, a vaporization of $\left(\frac{6\cdot0174+5\cdot9990}{2}=\right)$ 6·0082 pounds of water from the temperature of 100° Fahrenheit per pound of coal. The weight of water vaporized per hour from this temperature was, for the average of the two experiments, 5052 pounds.

The Sinclair boiler, when vaporizing (experiment E) 4851 pounds of water per hour from the temperature of 100° Fahrenheit, or nearly the same, and burning the same Scotch coal at the rate of 16·6076 pounds per hour per square foot of grate-surface, gave a vaporization of 7·3946 pounds of water per pound of coal; consequently, when supplying equal weights of steam in equal time with the same coal, the Lancashire boiler was $\left(\frac{7\cdot3946-6\cdot0082\times100}{7\cdot3946}=\right)$

18·75 per centum less economical than the Sinclair boiler, exhibiting in a striking degree the economic advantage of a high proportion of heating- to grate-surface, and a low rate of combustion; in other words, of employing an apparatus large in proportion to the work to be done. It also exhibits how great may be the errors in comparing the cost of the indicated horse-power in pounds of coal consumed per hour, for engines supplied with steam from boilers of different types and proportions, burning coals of widely varying vaporizing power with widely varying rates of combustion. Such comparisons can be only for the systems as a whole, and give no information as to the economic efficiency of any part. Those can only be comparative experiments which are made under comparable conditions, and the whole of the data—every fact influencing the final result—must be ascertained and stated separately.

Untrustworthiness of Measuring the Economic Development of the Power by the Heat acquired by the Condensing-Water.—Many persons have advocated using the number of units of heat acquired by the condensing-water in an hour per indicated horse-power, as the measure of the economy of the engine; but the results of these experiments show this method to be incorrect in principle, even when the thermal units due to the development of the power are added. Such comparisons will not be accurate, either absolutely or relatively. In these five experiments, all by the same observers with the same instruments, the discrepancies range from 6·6372 to 22·6336 per cent. (line 62), and prove how little trustworthy are results from this process.

Economy of the Feed-Water Heater.—When the feed-water heater was in use, during experiments A and B with the Sinclair boiler, the temperature of the feed-water was raised about 100° Fahrenheit, or from 100° to 200°, equivalent to an economic gain in fuel of about 9 per centum. The surface of the heater exposed to the gases of combustion in the flue behind the boilers, is not given, but the temperature of those gases exceeded 800° Fahrenheit, as they came from the Lancashire boilers as well as from the Sinclair boiler. Had the heater been exposed to the temperature of the waste gases from the latter boiler alone (400° to 450° Fahrenheit), nothing like this 9 per centum of economy would have been realized.

Condensation of Steam in the Steam-Jackets.—The mean results from experiments A, B and C, show, that of the total weight of steam evaporated in the boiler, $3\frac{1}{3}$ per centum was condensed in the steam-jackets of the cylinder. This per centum is greatly less than the truth—probably one-eighth less—owing to the causes hereinbefore stated. Now, as the jackets were supplied with boiler-steam by a small independent pipe, and had no communication with the main steam-pipe, no water could have been primed over into them, and the whole of what was found in them must have been due to the absorption of heat by the steam within the cylinder, for the insignificant quantity of heat lost by external radiation from the small and well protected surfaces of the jackets, need scarcely be allowed for. This absorption of heat from the jackets by the working steam within the cylinder, must have effected some economy in the production of the power.

MEMOIRS ON THE LIQUEFACTION OF OXYGEN, THE LIQUEFACTION AND SOLIDIFICATION OF HYDROGEN, AND ON THE THEORIES OF THE CHANGES IN CONDITION OF BODIES.

By R. Pictet.

IV.—Calculation of the Density of Liquid Oxygen.

MAXIMUM TENSIONS.

[Continued from Vol. lxxvi, page 165.]

Measurement of the Temperatures.—In a preceding chapter we gave a general formula, which we had derived, directly by analysis, from the mechanical theory of heat. This formula makes the maximum pressures dependent on the corresponding temperatures. It comprises besides, in its factors, the latent heat of volatilization at a temperature taken as a base, as well as the difference $c - k$, of the specific heat of the liquid, less that of the vapor. This formula is

$$l\left(\frac{P'}{P}\right) = \frac{[\lambda' + (c-k)(t'-t)]\ 431 \times 1{\cdot}293\ \delta \times 274\ (t'-t)}{10333\ [(274+t')^2 - (274+t')(t'-t)]}.$$

In this formula the following quantities are supposed to be known,

P' at t', λ', $(c-k)$ and δ.

As the analytical demonstration of this formula has not as yet been published, we do not think ourselves authorized to use it without

having verified it by at least one example, as applied to water, all of whose properties have been determined with so much care by M. Regnault.

We by no means wish to substitute an example for a demonstration, but a numerical verification of this nature will have the double advantage of showing in detail the method of using this formula, and the perfect agreement of the numerical results.

We will thoroughly discuss this subject in a forthcoming work, which is not as yet completed.

For the numerical verification of the formula, let us take the following data as given in the tables of M. Regnault: $t'° = 200°$; $t = 82°$; P' corresponding to $t = 11688{\cdot}96$ millimetres of mercury; δ, the density of the vapor of water, is given by the curve of co-volumes $= 0{\cdot}62665$; $\lambda' = 464{\cdot}3$ calories; $c - k$, according to M. Regnault, between 200° and 82° $= 0{\cdot}544$.

We have thus arbitrarily chosen a sufficiently great difference as

$$t' - t = 118 \text{ degrees.}$$

We have to solve the numerical equation:

$$l\left(\frac{11688{\cdot}96}{x}\right) = \frac{[464{\cdot}3 + 0{\cdot}544 \times 118]\ 431 \times 1{\cdot}293 \times 0{\cdot}62665 \times 274 \times 118}{10333\ [(274 + 200)^2 - (274 + 200)\ 118]}.$$

Performing this calculation with the aid of logarithms, we arrive at the following result:

l. Numerator	$= 9{\cdot}7747759$
l. Denominator	$= 9{\cdot}2414547$
Difference,	$0{\cdot}5333212$
l. Modulus,	$0{\cdot}3622157$
	$0{\cdot}1711055$ is log of $1{\cdot}482871$
Log P' =	$4{\cdot}0677740$
	$1{\cdot}482871$
	$2{\cdot}5849030$ is log of $384{\cdot}51$ mm.
x =	$384{\cdot}51$

If we now take the tension of the vapor of water, corresponding to 82°, from M. Regnault's tables, we will find it equal to 384·435 mm. This example suffices to show that our formula represents, with great exactitude, the function which makes the temperature dependent on the pressure.

We intend employing this formula, as applied to sulphurous acid, as a substitute for the readings of the alcoholic thermometer. In this manner, we will obtain dynamometric thermometers of great sensitiveness, which may be substituted for the entirely unreliable readings of all kinds of thermometers when exceeding —20° or —25°.

The complete solution of the numerical calculations rendered necessary by this transformation, is not as yet finished, as we wish to obtain several experimental verifications regarding imperfectly understood properties of the volatile liquids employed. To a certain extent we are therefore still obliged to fall back on the alcoholic thermometer. M. Salleron, of Paris, has made ten for me, graduated to — 100°, which I have used.

I have found that carbonic acid is transformed into a vapor under a pressure of 760 at a temperature of — 80°.

As our pumps can produce an almost perfect vacuum over this solid carbonic acid, the thermometers will no longer suffice, even for an approximate knowledge of the temperature produced; the thermodynamic formula must be employed.

In order to determine the value of λ' for carbonic acid at — 80°, I have employed two formulæ, previously demonstrated in a former publication already cited.[1] The first is

$$\lambda^{-80} = \frac{10333\ (274 - 80)^2}{1{\cdot}293\delta \times 431 \times 274} \times \left\{ \begin{array}{l} \text{A factor derived from the pres-} \\ \text{sures with regard to temperatures.} \end{array} \right.$$

The second gives the value of this derived quantity, which we will call D; it is

$$D = \frac{0{\cdot}041 \times 274}{274 - 80}.$$

Carrying out the numerical calculation and substituting for D, its value, we find for λ^{-80}:

$$\lambda^{-80} = 96{\cdot}766 \text{ calories.}$$

We have taken for the density at 0°, $\delta = 1{\cdot}5241$ (Regnault), λ' once known, we made the calculations for the curve represented in Plate II, by means of our general formula giving to t a series of values from 10 to 10 degrees. The abscissæ of this curve represent the temperatures, and the ordinates, the corresponding pressures.

Therefore, if the pressures indicated by the mercurial manometer be known, when a vacuum is established over the carbonic acid, we

[1] *Archives des Sciences phys. et. nat.* Janvier, 1876.

can determine the temperature of this body with the aid of this curve. There are many elements in this determination which are but imperfectly known, many figures to be corroborated by direct experiments, we therefore only advance this measurement of the temperatures "as a first approximation."

The following is the calculation for the temperature — 120°:

$$\lambda' = 96{\cdot}766 \text{ calories.}$$

$(c - k) = 0{\cdot}1947$ deduced from analogy of isomorphous bodies.

lg 104·554 = . .	2·0193407
lg 1·5241 = . .	0·1830135
lg 1·293 = . .	0·1115985
lg 431. = . . .	2·6344773
lg 274· = . . .	2·4377506
lg 40· = . . .	1·6020600
lg numerator = . .	8·9882406
lg 10333 = . .	4·0142264
lg $(194^2 - 194 \times 40)$ = . .	4·4753225
lg $\frac{1}{m}$ = . . .	0·3622164
lg denominator = . .	8·8517646
lg quotient = . .	0·1364760 log of 1·36923
lg 760 = . . .	2·8808136
	1·36923
	1·5115836 log of 32 mm. 488

The pressure corresponding to — 120° is 32·488 millimetres of mercury.

By means of identically the same calculations, the curve *A* of the tensions of carbonic acid has been found.

We have made similar determinations for protoxide of nitrogen. The results are given by curve *B*, calculated from 10° to 10°.

As a starting point, we took a pressure of — 92° with the barometric pressure of Geneva, which is 730 millimetres. That is, the temperature of ebullition of this liquid, determined by observation.

The two curves, *A* and *B*, show that the protoxide of nitrogen has greater tension than carbonic acid. We can therefore attain lower temperatures with the protoxide of nitrogen than with carbonic acid.

Our pumps *O* and *O'*, which exhausted the vaporized carbonic acid in tube *D* (Fig. 1), produced a vacuum of 9 millimetres. The pres-

sures oscillated between 8 and 12 millimetres, according to the speed of the pumps. We may therefore infer, that "the temperature corresponding to the first three experiments, made with carbonic acid, was about — 130°; and — 140° for the last two, made with protoxide of nitrogen."

From these results we find that the maximum tension of liquid oxygen is at

— 130°	pressure, . .	273 atmospheres.
— 140°	" . .	252 "

A difference of about 10° would produce an increase of pressure of 21 atmospheres.

Such are for the present the numerical conclusions furnished by these first experiments.

We stated previously, that the protoxide of nitrogen gave us different pressures from those given in works on physics. We noticed it in this manner:

While making the preliminary investigations which led us to the mechanical apparatus employed, in order to determine which of the two liquids should be chosen to obtain the greatest cold, I compared the two following tables, which give the maximum tensions of carbonic acid and of protoxide of nitrogen.

Temperature.	Carbonic Acid.	Protoxide of Nitrogen.	Difference.
— 87·2		1 atmos.	
— 84·4		1·10	
— 81·7		1·22	
— 80·	1 atmos.		
— 78·9		1·37	
— 77·2	1·36		
— 76·1		1·55	
— 73·3		1·77	
— 70·5	2·28	2·03	+ 0·25
— 67·8		2·34	
— 63·9	3·6		
— 62·2		3·11	
— 59·4	4·6	3·58	+ 1·02
— 51·	7·	5·36	+ 1 64
— 48·8	7·7		
— 48·3		6·09	
— 37·2		9·74	
— 36·6	12·5	9·91	+ 2·59

An examination of this table shows that the pressures are nearly the same for both liquids; however, between the temperatures of

— 75° and — 36 6°, "the tensions of protoxide of nitrogen are always less than the corresponding ones of carbonic acid." This decided me, at first, to employ carbonic acid, which, according to this table, should produce the greatest cold in a vacuum. For the formula which allies the tensions to the temperatures, shows that if a liquid at any temperature possess a higher tension than another liquid at the same temperature, this difference will very probably exist for all temperatures with the same sign. As temperatures of — 35° to — 50° are much more reliable than those indicated by alcohol at — 75° to — 80°, I felt constrained to place more confidence in the pressures indicated at these higher temperatures. But, on substituting pure protoxide of nitrogen, made from crystallized nitrate of ammonia, for carbonic acid, during the last two experiments, I found for this body, pressures notably higher than those of carbonic acid. There is, consequently, a manifest error in the tables prepared by Faraday and other experimentalists. These differences are so marked, that I thought, for a while, that air had found its way in and increased the pressure of the condensing gas.

To be assured of the contrary, I condensed all the contents of the gasometer, in the tube *S*, then I distilled it. The vapors slowly re-entered the gasometer. After having thoroughly emptied it, so as to remove every trace of a foreign gas, the pressures indicated by the manometer corresponded exactly to the maximum tensions of liquid protoxide of nitrogen. The pumps not working, the temperature indicated by the alcoholic thermometer afforded a means of following, degree by degree, the continued elevation of temperature. This method presents the decided advantage of allowing a long-continued observation. The temperature indicated by the thermometer is certainly equal to or but slightly higher than that of the protoxide of nitrogen; consequently, the indications of the manometer are, in all cases, *minima*.

We have prepared the following table for carbonic acid and protoxide of nitrogen:

Temp.	Carb. Acid.	Prot. Nitrog.	Differences.
— 92°		1 atmos.	
— 90°		1·10	
— 88°		1·15	
— 86°		1·35	
— 84°		1·40	
— 82°		1·72	
— 80°	1 atmos.	1·90	— 0·90

Temp.	Carb. Acid.	Prot. Nitrog.	Differences.
— 78°	1·15 atmos.	2·12 atmos.	— 0·97
— 76°	1·33	2·36	— 1·03
— 74°	1·55	2·60	— 1·05
— 72°	1·78	2·85	— 1·07
— 70°	2·08	3·15	— 1·07
— 68°	2·41	3·50	— 1·09
— 66°	2·71	3·80	— 1·09
— 64°	3·10	4·20	— 1·10
— 62°	3·45	4·60	— 1·15
— 60°	3·90	5·05	— 1·15
— 58°	4 40	5·50	— 1·10
— 56°	4·90	6·10	— 1·20
— 54°	5 46	6·32	— 0·86
— 52°	6·10	7·05	— 0·85
— 50°	6·80	7·63	— 0 83
— 48°	7·40	8·30	— 0 90
— 46°	8·05	9·00	— 0·95
— 44°	8·72	9·60	— 0·88
— 42°	9·45	10·30	— 0 85
— 40°	10·25	11·02	— 0·77
— 38°	11·00	11·70	— 0·70
— 36°	11·90	12·50	— 0·60
— 34°	12·70	13·19	— 0·49

This table, prepared immediately after repeated experiments, shows us that protoxide of nitrogen always has a higher tension than carbonic acid, which agrees with the theoretical inferences we have drawn. We give this table *in extenso* as the differences between the pressures given in works on physics are too great, and correspond to temperatures too far apart. We also desired to compare carbonic acid and protoxide of nitrogen, directly and under identically the same conditions, with each other.

The general table of our experiments, as given at the end of last chapter, furnishes us with still another absolute proof of the liquid state of the oxygen.

In three experiments (Nos. 2, 4 and 5), the third jet was of but short duration, and I closed the regulating cock quite suddenly. This allowed a small quantity of liquid oxygen to adhere to the sides of the tube.

The rapid evaporation lowered the temperature of the sides several degrees, while the vapors formed escaped through the cock. At the moment of closing, the pressure was 245 atmospheres in the second

experiment. Several instants later, it rose to 253 atmospheres. This increase of pressure at a time when the production of gas has long ceased, can only be explained by admitting that a small quantity of oxygen passes back from the liquid to the gaseous state.

The conductivity of the copper sides of the tube is sufficient to very rapidly restore an equilibrium of temperature. In the two other experiments, the pressure rose from 213 to 215 atmospheres, and from 205 to 212 atmospheres.

Solidification of the Oxygen.

In the third and fourth experiments, we used the electric light, condensed by a reflector, to examine the appearance of the issuing jet. This intense light enabled us to clearly distinguish two parts in the fluid stream. A central portion partially transparent of 2 to 3 mm. diameter, and a surrounding envelope of from 10 to 12 mm., of a brilliant white. These two concentric envelopes resembled two cylinders, one inside the other. The exterior one seemed to consist of a dust, like chalk or snow. Their length was about 10 to 12 centimetres. We endeavored to determine the nature of this white substance, to know if it might be solidified oxygen.

In this research we made use of polarization. By examining light reflected at a right angle with regard to the incidental rays, with a Nicol's prism, it is easy to determine whether or no it is polarized. The reflector placed 2 metres from the orifice of the jet, threw horizontal rays on the oxygen. M. H. Dufour, professor of physics at the Academy of Lausanne, had the kindness to examine the light reflected by the jet; placing himself so that the rays observed in the analyzer were emitted perpendicularly to the axis of the parabolic reflector. During the jet the Nicol prism was turned 90°; during its rotation, M. Dufour observed distinctly a notable difference in the intensity of this light, which proves that it was partially polarized, at least. This experiment should be repeated several times, and would seem to indicate that the oxygen which issues from the tube, evaporates with such energy that the liquid particles are transformed into small solid crystals, a real dust of frozen oxygen. The heat of the surrounding air almost instantly retransforms these crystals into gas.

This is the general character of the different results furnished us by these experiments. They are still quite incomplete and demand

verification, and especially corroboration by other allied researches. We must endeavor to render this liquid oxygen visible, by condensing it in a transparent receptacle. The problem is a very complicated one, bristling with mechanical difficulties.

We must prevent the natural deposit of hoar frost on the cold surfaces, which mars their transparency. We must have tight joints with fragile bodies, etc., etc. Finally, we will do what we can to circumvent all these obstacles.

(To be continued.)

ON THE DEVELOPMENT OF THE CHEMICAL ARTS, DURING THE LAST TEN YEARS.[1]

By Dr. A. W. Hofmann.

From the *Chemical News*.

[Continued from Vol. lxxvi, page 173.]

Where circumstances admit, the animal charcoal, after being used for clarification, is incinerated, and the ash is further utilized. Or the recent bones are treated with hydrochloric acid to extract the phosphates, and the residual cartilage is used in the glue manufacture. Upon this H. Fleck founded a very rational process[2] which A. W. Hofmann has already discussed in Report, but which does not yet appear to have been universally introduced, though the objections raised on account of the want of suitable vessels for the evaporation of the lyes might not be insuperable.

Similar has been the fate of Metrand's ingenious proposal to act with hydrochloric acid gas upon an intimate mixture of equal parts of bone-ash and wood-charcoal, thus obtaining phosphorus, a method also mentioned by Hofmann. Instead of bone-ash a natural phosphate might serve, or one prepared by precipitating the hydrochloric solution of bones with lime. The chloride of calcium thus obtained could be used without difficulty for the reproduction of hydrochloric acid, so that the final result would be sulphate of lime containing a small

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1 "Berichte über die Entwickelung der Chemischen Industrie während des letzten Jahrzehends."

2 "Verbessertes Verfahren der Phosphor Fabrikation." Hugo Fleck. Leipzig, 1855. See *Wagner Jahresber.*, 1855, 66.

proportion of phosphates, which might be very well utilized as manure.

Recently the process suggested years ago by Wœhler to decompose phosphate of lime with silica and carbon has been carried out on the large scale at Coignet's works in Lyon, having been rediscovered by Aubertin and Robiquet.[i] We must not forget that Brand first obtained phosphorus by distilling urine previously to dryness with sand.

Brisson has modified Wœhler's process by adding soda to the above-mentioned mixture, and heating in shaft-furnace. The operation must be managed in such a manner that the liberated phosphorus may not come in contact with the air, which is not very easily arranged.[ii]

In England, sombrerite, which is found abundantly in the Antilles, and especially in the island of Sombrero, is said to be used for obtaining phosphorus. In consists of 49·6 phosphoric acid, 42·7 lime, 6·5 alumina, and 1·1 moisture.[iii]

Another West Indian mineral known as Redonda phosphate is used in England, consisting chiefly of alumina and phosphoric acid, along with some iron. Spence opens up the mineral, after ignition and pulverization, by boiling with sulphuric acid. By the introduction of ammonia from gas-liquors ammonia-alum is formed and readily removed by crystallization. The mother-liquor contains principally phosphate of ammonia, which may be used in the manufacture of manure.

Gerland extracts bones, previously deprived of fat, with sulphurous acid, and heats the solution when the phosphates are deposited, whilst the acid escapes and may be re-used.[iv] This process appears to be in use in France, where patents for its execution have been taken out by several manufacturers.[v]

As far as the reporter is aware the total supply of phosphorus is furnished at present by two establishments only; the firm of Albright

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[i] *Moniteur Scientif.*, 1869, 173.

[ii] *Wagner Jahresber.*, 1869, 223.

[iii] *Wagner Jahresber.*, 1862, 247. An attempt has also been made to use phosphate of iron instead of phosphate of lime in the preparation of phosphorus. Details are not known and success is improbable.

[iv] *Bul Soc. Chim.*, ii, 1864.

[v] *Zeitscher. f. Ch.*, 1870, *Journ. f. Prakt. Ch.*, iv, 67, 1870.

and Wilson, of Oldbury, near Birmingham, and by Coignet and Son, of Lyon. O. Pauli, who formerly managed the works of Schattenmann, at Buxweiler, near Strasburg, produced there, and subsequently in his own manufactory at Karlsruhe, phosphorus of excellent quality, and employed Seubert's ingenious process for drawing the product into rods. According to recent intelligence the manufacture there has been abandoned, and in general no phosphorus is produced in Germany, a fact evidently depending on the price of the raw material.

In Austria, also, phosphorus is at present no longer produced. Ploy formerly fitted up a manufactory at Obernberg, in Upper Austria, and subsequently, in 1849, at Manning, in the same province; but the production, which amounted to 250 cwts. annually, ceased in 1865. Another establishment was opened in 1853 by Baron Riese-Stallburg, at Wranowitz, in the circle of Pilsen, in Bohemia, where about 180 cwts. of phosphorus were yearly produced. This manufactory also was discontinued in 1865.

The firm of J. D. Stark, who, in 1847, introduced the manufacture of phosphorus on a large scale at their works at Kasnau, in Bohemia, abandoned it in 1868. In a pamphlet issued by opportunity of the Exhibition, under the title "The Firm J. D. Stark, their Mines, and Manufactories of Mineral," compiled by A. Prochaska, manager of the works at Kasnau, the reason alleged was the high price of bones, due to the great development of the sugar manufacture in Austria, and especially in Bohemia, and to their consumption in agriculture both as bone-dust and in artificial manures. A second reason why the establishment could not compete with England in the price of phosphorus, might possibly be found in the circumstance that Messrs. Stark obtained from 25 to 26 cwts. of bones only 1 cwt. of phosphorus, *i.e.*, 4 per cent., while in England, 8 per cent. are extracted. The cause of this loss was that the gases on their exit from the retort, had to overcome a pressure, which, though slight, was sufficient to facilitate the escape of the vapor of phosphorus through the porous sides of the retort.

The applications of phosphorus are so numerous, that it is scarcely possible here to enumerate them all. Its greatest consumption by far is in the manufacture of matches. But it is difficult to form a correct estimate of the extent of this application. If we take as a basis the statements *Compagnie Generale des Allumettes Chimiques*,

according to which the consumption in France is 360 tons, and if we assume that in the rest of Europe only double the quantity is used for the same purpose, it appears that the match trade requires the serious amount of 1080 tons, = 1,080,000 kilos. This calculation, however, is merely approximate. Nor are we able to check it either by the number of matches yearly produced or by the quantity of the inflammable mass used in their preparation, since both these factors are unknown. As regards the amount of the inflammable mixture, especially, accounts vary very widely, its proportion of phosphorus being given as low as 6 and as high as 40 per cent. Thus, according to M. Hochstätter, of Langen, 15 grms. of mass containing 7 per cent. of phosphorus suffice for 1000 matches, whilst according to M. Pollak, 31 grms. of a mass with the same percentage of phosphorus are required for the same. These discrepancies are intelligible if we consider the different sizes of the matches and the unequal bulk of their heads. As to the above-mentioned quantity, of 1080 tons of ordinary phosphorus, the amorphous must be added, which is consumed to a not inconsiderable extent, for the Swedish safety-matches, and also the comsumption for other purposes, the amount of phosphorus yearly required may be estimated at 1200 tons, for the production of which 15,000 tons of bones are consumed, if the yield is assumed at 8 per cent.

The Match Trade.—No fewer than 40 firms exhibited matches at Vienna, but not one of them has produced any decided novelty. The attempts of manufacturers have been almost exclusively directed to a pleasing appearance, the increase of production, and the reduction of price. The great bulk of matches are still made with ordinary phosphorus. Next in number, though at a great distance, follow those that can be struck only on a peculiar surface prepared with amorphous phosphorus.

Matches tipped with a mixture containing amorphous phosphorus, and capable, therefore, of being ignited on any surface, were exhibited only by one firm, H. Hochstätter, of Langen, near Frankfurt on the Main.

Matches of this kind were even displayed at the London Exhibition of 1851, but met with no approval as they were not easy to strike, and their ignition was often attended with a slight explosion and the projection of sparks. The matches of this kind exhibited by the

Viennese firm of Forster & Wawra at Paris, in 1867, still suffered from the same defects, though in a smaller degree.[i]

H. Hochstätter has finally succeeded in the complete solution of the problem, which the present reporter attempted twenty six years ago, as may be seen from the report on the matches displayed in the London Exhibition of 1872, to which he therefore refers.[ii]

The reporter, at an early date, had proved by his researches on the remarkable behavior of amorphous phosphorus, with chlorate of potash and other bodies rich in oxygen, that, with phosphorus in this modification, mixtures of every degree of combustibilty might be prepared. Among the matches which were experimentally prepared with such mixtures there were always a few which satisfied every requirement. Though the technical problem was thus by no means solved these experiments had at least proved the possibility of such a solution. To M. Hochstätter belongs the merit of having discovered the conditions for the industrial preparation, with amorphous phosphorus, of matches capable of superseding those containing the ordinary quality of phosphorus, even in the opinion of a prejudiced public, and which were capable of ignition on any at all suitable surface. The Hochstätter matches, indeed, leave nothing more to be wished for. They can be struck upon cloth; they burn quietly, without noise or spirting, almost without smoke and smell, and very rarely fail, as the reporter has convinced himself by a prolonged trial.

These matches attract absolutely no moisture, and can, therefore, be used on shipboard and in all climates. What is still more important, the workmen during their production are not exposed to danger of any kind soever; nor can they give rise to poisoning, intentional

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[i] The firm of B. Forster & F. Wawra is, since 1871, known as F. Wawra & A. Kempny. It is unfortunate that this firm, one of the oldest in Austria, and the direct successor of Th. Preschel, did not exhibit. The works occupy a surface of 2381 square metres, employ 160 persons, and produce yearly 2500 millions of matches. The dextrin required in the mixture is made on the premises. The establishment has ceased making matches with amorphous phosphorus, as the demand was insufficient.

[ii] Space does not allow of a repetition of the history of the match trade. It may be found in A. W. Hofmann's "Report" for 1862, that of the writer for the same occasion: in the copious "Report" of Dr. Holdhaus on matches in the Paris Exhibition of 1867,; and in the "Report" of the General Direction on Matches and Explosives at the Vienna Exhibition of 1873. See also Thiel's paper in *Wagner's Jahresber.*, 1866, 747.

or accidental. They are also cheaper than the ordinary kind, since the mixture costs:—For those with sulphur, 0·8 shilling; for those without sulphur, 1·7 shillings; for wax tapers, 1·42 to 2·0 shillings; whilst it is stated, that, in Germany the mixtures for the same number of ordinary matches, cost respectively 1 shilling, 2 shillings, and 2·8 shillings.

The manufacture of these matches is, without doubt, the greatest improvement which has been effected in this trade since 1862, and the question now arises, in how far it has become the serious duty of governments to listen to the many and important voices, which for years, though unfortunately in vain, have urged the banishment of ordinary phosphorus from the match trade. Half measures—such as improved ventilation in factories, and the prohibition of glue in the composition of the mixture, because in that case it must be used hot—remove the evil only partially, or not at all. An attempt has been made to lessen the danger to which the workmen are exposed by reducing the proportion of phosphorus to a minimum. How effectual such endeavors have hitherto been, appears from the circumstance that in some works even yet, mixtures are employed with a proportion of phosphorus far exceeding the 6 or 7 per cent., which in Austria is found amply sufficient.

Dr. Letheby proposed the use of oil of turpentine for the absorption of the vapors of phosphorus, and in the works of Black & Bell, at Stratford, near London, and more recently in several German establishments, each workman wears on his breast a vessel containing this oil. But this suggestion has, by no means, removed the pernicious effects of the vapor of phosphorus, since absorption still takes place through the skin and the clothing. It is also still an open question, whether the vapors of phosphorus are rendered harmless by oil of turpentine, or if their smell is merely disguised. Such palliatives are often hurtful instead of useful. Those concerned allow themselves to be lulled into a feeling of security whilst the danger is still present.[1]

When governments find it necessary to interfere in technical arrangements, great caution is doubtless requisite, since an ill-timed regulation may increase certain evils instead of diminishing them. The question, however, now a perfect method of employing amor-

[1] See W. Jettle, "Die Zündwaaren Fabrikation in ihrer Gegenwärtigen Ausbildung." Brunswick: F. Vieweg. 1871.

phous phosphorus in the manufacture of matches has been discovered, seems analogous to that of the poisonous Schweinfurt green, which governments who care for the physical well-being of their citizens, did not hesitate to prohibit when once a complete substitute had been found in the shape of the new chrome green. Moreover, the opposition of manufacturers to the introduction of amorphous, in place of ordinary, phosphorus, would not be very violent since in an establishment which employs the common kind of phosphorus, little alterations of plant would be needful.

Ordinary phosphorus can lay claim neither to economy, nor to a superior quality of the article produced, nor to greater convenience in the manufacture. On the contrary, quite independent of its absolute innocence in all these points, the amorphous modification has the advantage. In laying out new works, there is further no occasion for costly and especial arrangements for ventilation, and for abnormally lofty work-rooms if the amorphous phosphorus is to be used.

The *Compagnie Générale des Allumettes Chimiques*, which, by a recent French law, has obtained a monopoly of the match trade, will, it is to be hoped, not fail to set in this respect a good example, as, on account of the concentration of the whole manufacture, it would be easy for it to work this improved process to advantage.

As to the composition of the mixture used in Sweden, for so-called safety-matches, which ignite only on a prepared surface, there prevails still some uncertainty, and it is possible that the formulæ used in different manufactories are not absolutely identical. According to Jettel, whose statements are based upon an analysis of the mixture performed by Kriwanek, in the laboratory of Prof. Hlasiwetz, but has been slightly modified for actual use, its composition is the following:—

Glass,	$1\frac{1}{4}$ parts.
Glue,	1 part.
Bichromate potash,	$\frac{4}{5}$ "
Chlorate potash,	$6\frac{1}{4}$ parts.
Oxide of iron,	$\frac{1}{2}$ part.
Black oxide of manganese,	2 parts.
Sulphur,	1 part.

According to Jettel, the quantity of sulphur is given too high, the cause of which may possibly be that the matches were not genuine

Swedish. Jettel considers that it should not exceed the half of the proportion as given above.

The composition of the friction-surface is:—

Glue,	$1\frac{1}{2}$ parts.
Umber,	1 part.
Manganese,	$4\frac{1}{2}$ parts.
Sulphuret of antimony,	$16\frac{2}{3}$ "
Amorphous phosphorus,	10 "

Gentele has subsequently examined the composition of Swedish matches, and obtained very different results. He found:—

Chlorate of potash,	32 parts.
Bichromate of potash,	12 "
Red lead,	32 "
Sulphuret of antimony,	24 "

This mixture ignited readily upon a surface consisting of 8 parts amorphous phosphorus, with 9 parts of sulphuret of antimony.[1]

It appears from the experiments of Gentele, that slight differences in the mixtures, have little effect upon the quality of the matches, if only the preparation has been careful, *i. e.*, if the separate ingredients have been ground as finely as possible, and intimately mixed, which applies also to phosphorus matches, and to all similar mixtures. The selection and preparation of the wood are not a matter of indifference.

(To be continued.)

THE "CAMEL" ENGINE OF ROSS WINANS.

By J. Snowden Bell.

There are two American locomotives that have become historic from having given origin and name to the two classes of engines which have, to a greater extent than any others, contributed to the development and economization of railroad freight transportation. These are the "Camel," built by Ross Winans, of Baltimore, in June, 1848, for the Baltimore and Ohio Railroad, and the "Consolidation," which M. W. Baldwin & Co., of Philadelphia, constructed for the Lehigh Valley Railroad, in July, 1866. The performance of the latter was found to be so satisfactory, that the class of engines of

[1] Gentele, *Dingl. Pol. Journ.*, ccix, 369.

which it is a type, has been adopted as their standard heavy freight engine by the Baltimore and Ohio, Pennsylvania, Erie, Lehigh Valley and other leading railroads in the United States, and two "Consolidation" engines of the largest size, have recently been shipped by the Baldwin Locomotive Works, to Australia.

While there is obviously a wide range of difference, both as to design and detail, between the improved "Consolidation" engine of 1878, and the original "Camel" of 1848, the boldness and originality of conception, and the elements of practical value, presented at the early date of the latter, have given the "Camel" engines a prominent position in the history of American locomotive engineering, the record of which derives additional interest from the fact that their general principles, modified and improved by modern practice, are to be found embodied in the most successful type of freight engines now in use.

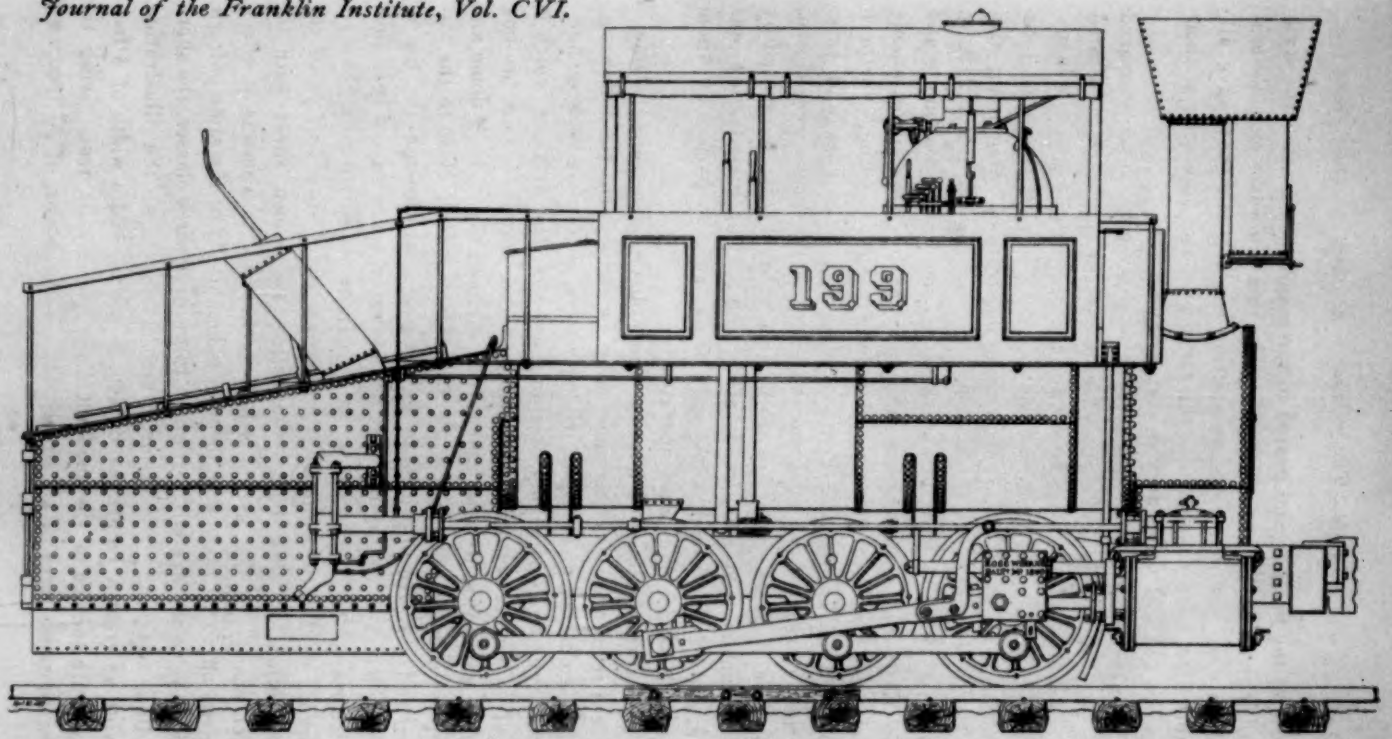
The first locomotives constructed in the United States with more than one pair of driving wheels, were the "grasshopper" engines of Phineas Davis, in 1834, one of which was exhibited at the Centennial Exposition. These engines had four coupled wheels of small diameter, driven from a counter shaft geared to a driving shaft rotated by two upright cylinders, and were succeeded by the "crab" engines of Gillingham & Winans, which were of the same general type, saving that they were of greater size and weight, and were provided with horizontal instead of vertical cylinders. The inadequacy of the "crabs" to fulfil the increasing demands of freight service upon heavy grades, developed the American eight-wheel connected engine, the first one of which, the "Buffalo," was built by Ross Winans, for the Baltimore and Ohio R. R., in November, 1844, and five others of the same pattern, known as "mud diggers," were made by him for the same road, between that date and December, 1846. It is not maintained that Mr. Winans was the *inventor* of the eight-wheel connected engine, but he was, without doubt, the first constructor of such an engine in the United States. M. W. Baldwin, of Philadelphia, built his first six-wheel connected engine in 1842, and it was not until 1846 that he commenced the construction of eight-wheel connected engines.

The six "mud digger" engines built by Winans, had the ordinary horizontal boiler, cylinders 17 x 24 inches, and eight driving wheels, 33 inches in diameter, coupled to a counter shaft placed immediately

in rear of the fire-box and geared to the engine shaft, after the style of Gillingham & Winans' "crabs." Up to a few years past, several of them were yet in service as shifting engines, but they have all, by this time, been consigned to the scrap pile, although some of their cylinders are still doing duty in stationary engines.

The inherent defects of this type of engine, specially as compared with the Baldwin six and eight-wheel connected engines, necessitated the production of a more efficient and practical machine, and Winans' answer to this requirement was the "Camel" engine, the latest form of which is shown in the accompanying illustration. Beginning with the engine "Camel," in June, 1848, and ending with No. 219, in February, 1857, he built 119 of these engines for the Baltimore and Ohio Railroad, as well as a large number for other roads, among which were the Pennsylvania, Philadelphia and Reading, Northern Central, Cumberland and Pennsylvania, and New York and Erie. At the outbreak of the civil war, his shops in Baltimore were closed, and three of these engines in stock were subsequently purchased by the Baltimore and Ohio road, after which their manufacture was practically discontinued, a few only being made by parties by whom the shops were a short time operated.

Among the features of novelty which distinguished the "Camel" engines from prior constructions, may be enumerated the following: 1. The employment of eight driving wheels, set closely between horizontal cylinders and a long overhung fire-box, the width of which is equal to or greater than the distance over frames. 2. A fire-box having a downwardly and rearwardly inclined top. 3. A dome and engineer's house, placed on the top of the boiler, close to the forward end. 4. An upper shute for feeding coal through the top of the fire-box. 5. A fire-box having no water space on its rear side, which was closed by doors, so as to expose its entire area when required. 6. The abandonment of crown sheet stay bars, and the substitution of stay bolts, connecting the crown sheet with the outer shell. 7. The use of a half-stroke cam, as a means of effecting cut off. All these engines were substantially of the same pattern, except as to the fire-box, of which there were three classes, the short, medium and long; the latter, which is shown in the illustration, having as great a length as 8 feet, 6 inches, and a width of 4 feet. The grate surface of the medium class was 6 x $3\frac{1}{2}$ feet, giving the then enormous area of 21 square feet. The boiler, of $\frac{5}{16}$ iron, was



D. A. PARTRIDGE, PHOTO-LITH. 21 S SEVENTH ST., PHILA.

J. Snowden Bell

THE CAMEL ENGINE OF ROSS WINANS.

46 inches in diameter. The cylinders (except in a few of the earlier engines, which were only 17 inches) were 19 inches in diameter, and 22 inches stroke, and the diameter of the driving wheels, in all cases, was 43 inches, with an extreme wheel base of only 11 feet, 3 inches. The front and rear wheels only were flanged, and end play was left in the boxes, to admit of the passage of the engines around curves. Chilled cast iron tires were used, fastened by hook headed bolts, passing between the centre and tire, and, in the earlier engines, chilled wheels without separate tires were employed. The weight of the engines varied from 25 to 29 tons (of 2000 lbs.). The valve motion was of the old "drop hook" pattern, and the valves could be operated either by an eccentric or a half stroke cam for cutting off, as desired.

In regular service, these engines hauled eight loaded freight cars, 20 tons (of 2000 lbs.) each, up grades of 116 feet to the mile, with frequent curves of 600 feet radius, but they have, for a period of two months, performed the remarkable and probably unparalleled duty of hauling a tender and one loaded car up a grade of 1 in 10, or 528 feet to the mile, having curves of 300 and 400 feet radius, at a speed of 13 miles per hour. This was done upon a temporary track laid over the Kingwood Tunnel, on the Baltimore and Ohio Railroad, and Benjamin H. Latrobe, Esq., the engineer in charge of the work, states the total gross weight to have been 120,000 lbs., the total resistance and traction required to overcome it, 13,828 lbs., and the adhesion required $\frac{1}{4 \cdot 05}$ of the weight on drivers; further, expressing his belief that the limit of adhesion had been fully reached. Over another temporary track at the Board Tree Tunnel, having grades of 1 in 23 (229 feet to the mile), and curves of 300 feet radius, from sixty to seventy freight cars, and two passenger trains were daily taken by four "Camel" engines. Upon levels, and grades less than those before referred to, the loads hauled by these engines were equal to those of any other construction prior to the "Consolidation," and greater than most of them.

The inclined fire box was patented by Ross Winans, May 9th, 1854, and was substantially adopted by James Milholland, Master of Machinery of the Philadelphia and Reading Railroad, in two passenger engines built by him in 1859, since which time, all, or nearly all, of the engines built by that company have contained it. In 1875, the Pennsylvania Railroad adopted the "Consolidation" as their

standard heavy freight and helping engine, and prepared a design, differing, in some particulars, from that of the original engine, among which was the substitution of the Winans' inclined fire-box for the wagon top fire-box used in the Baldwin Locomotive Works construction. The system of staying the crown sheet by stay bolts, connecting it with the outer shell instead of crown bars, is also adopted in the Pennsylvania "Consolidation," and is, moreover, in the "Belpaire" furnace, coming into general use in continental Europe.

The imperfections of the "Camel," both in design and construction, were many, and the class is fast going out of existence, but it has done, and to-day is doing, good service. It first practically demonstrated the superiority of the eight-wheel connected engine for heavy traffic, and while for a time the ten-wheel engine met with greater favor, the revival of the eight driver type in the "Consolidation" class, evinces that thirty years' experience endorses that demonstration. It may be urged that the two-wheel truck of the "Consolidation" is an important factor, but in the view of the writer, this is questionable. In a ~~1,000,000~~ lb. engine it carries but 12,000 lbs., and engines of this class have run satisfactorily with it removed. In the consideration of the "Camel," relatively to the "Consolidation" type, it must be remembered that after due allowance for the lapse of time between them, and the perfection of detail and workmanship in the later engine due thereto, it is the comparison of a 60,000 lb. and a 100,000 lb. engine, and a 19 x 22 inch cylinder against a 20 x 24 inch. Each of them is a credit to its designer and his time, and the reviewer of thirty years hence will doubtless have before him a structure which will make the "Consolidation," like the "Camel," a thing of the past.

Heat of Molecular Rotation.—Hermann Herwig has examined the development of heat which arises in consequence of the rotation of molecular magnets, or of electrolytic molecules. The phenomenon was first observed by Joule, and has been attested by others, but no direct investigations have hitherto been instituted to determine its precise character. Herwig's experiments show that, after eliminating the influence of induction currents, there is a considerable quantity of residual heat, which is due to molecular rotation.—*Ann. d. Phys. u. Chem.* C.

A NEW SYSTEM OF ELECTRIC LIGHTING.

By Profs. ELIHU THOMSON and EDWIN J. HOUSTON, of the Philadelphia Central High School.

Having been engaged in an extended series of experimental researches on dynamo-electric machines, and their application to electric lighting, our attention has been directed to the production of a system, that will permit the use of a feebler current for producing an electric light than that ordinarily required, or, in other words, the use, when required, of a current of insufficient intensity to produce a continuous arc. At the same time, our system permits the use of a powerful current, in such a manner as to operate a considerable number of electric lamps placed in the same circuit.

As is well known, when an electrical current, which flows through a conductor of considerable length, is suddenly broken, a bright flash, called the extra spark, appears at the point of separation. The extra spark will appear, although the current is not sufficient to sustain an arc of any appreciable length at the point of separation.

In our system, one or both of the electrodes, which may be the ordinary carbon electrodes, are caused to vibrate to and from each other. The electrodes are placed at such a distance apart, that in their motion towards each other they touch, and afterwards recede a distance apart which can be regulated. These motions or vibrations are made to follow one another at such a rate, that the effect of the light produced is continuous; for, as is well known, when flashes of light follow one another at a rate greater than twenty-five to thirty per second, the effect produced is that of a continuous light. The vibratory motions may be communicated to the electrodes by any suitable device, such, for example, as mechanism operated by a coiled spring, a weight, compressed air, etc., but it is evident that the current itself furnishes the most direct method of obtaining such motion, as by the use of an automatic vibrator, or an electric engine.

In practice, instead of vibrating both electrodes, we have found it necessary to give motion to but one, and since the negative electrode may be made of such size as to waste very slowly, motion is imparted

to it, in preference to the positive. The carbon electrodes may be replaced by those of various substances of sufficient conducting power.

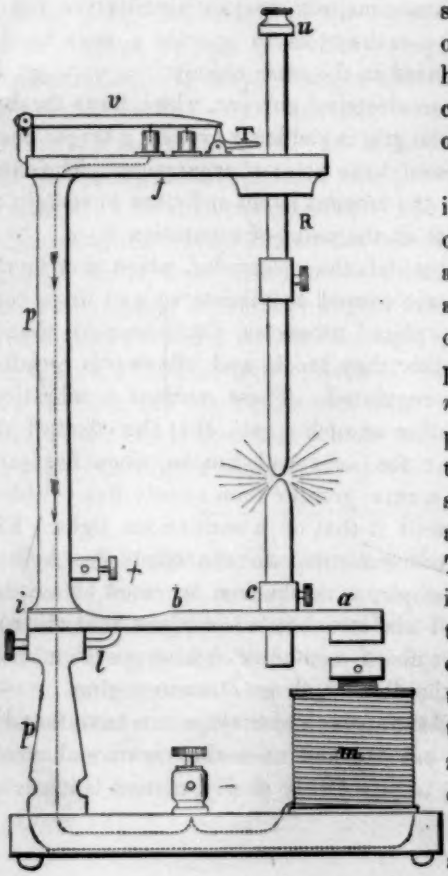
In this system, when desired, an independent battery circuit is employed to control the extinction and lighting of each lamp.

PHILADELPHIA, *Sept. 19th*, 1878.

A NEW ELECTRIC LAMP.

By Profs. ELIHU THOMSON and EDWIN J. HOUSTON, of the Phila. Central High School.

FIG. 1.



The following is a description of one of the forms of electric lamp which we have devised, to be used in connection with our system of electric lighting:

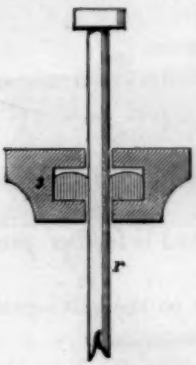
A flexible bar, *b*, of metal is firmly attached at one of its ends to a pillar, *p*, and bears at its other end an iron armature, *a*, placed opposite the adjustable pole-piece of the electro-magnet, *m*. A metal collar, *c*, supports the negative electrode, the positive electrode being supported by an arm, *j*, attached to the pillar, *p*. The pillar, *p*, is divided, by insulation at *i*, into two sections, the upper one of which conveys the current from the binding-post marked +, to the arm, *j*, and the rod, *r*, supporting the positive electrode.

The magnet, *m*, is placed as shown by the dotted lines,

in the circuit which produces the light. The pillar, *p*, is hollow, and has an insulated conducting wire enclosed, which connects the circuit-closer, *v*, to the binding-post, marked —. The current is conveyed to the negative electrode, through *b* and the coils of the magnet, *m*. When the electrodes are in contact, the current circulating through *m* renders it magnetic and attracts the armature, *a*, thus separating the electrodes, when, on the weakening of the current, the elasticity of the rod, *b*, again restores the contact. During the movement of the negative electrode, since it is caused to occur many times per second, the positive electrode, though partially free to fall, cannot follow the rapid motions of the negative electrode; and, therefore, does not rest in permanent contact with it. The slow fall of the positive electrode may be insured either by properly proportioning its weight, or by partly counterpoising it. The positive electrode thus becomes self-feeding.

The rapidity of movement of the negative carbon may be controlled by means of the rigid bar, *l*, which acts, practically, to shorten or lengthen the part vibrating.

FIG. 2.



In order to obtain an excellent but free contact of the arm, *j*, with the positive electrode, the rod, *r*, made of iron or other suitable metal, passes through a cavity, *s*, Fig. 2, filled with mercury, placed in electrical contact with the arm, *j*. Since the mercury does not wet the metal rod, *r*, or the sides of the opening through which it passes, free movement of the rod is allowed without any escape of the mercury. We believe that this feature could be introduced advantageously into other forms of electric lamps.

In order to prevent a break from occurring in the circuit, when the electrodes are consumed, a button, *v*, is attached to the upper extremity of the rod, *R*, at such a distance that when the carbons are consumed as much as is deemed desirable, it comes into contact with a tripping lever, *t*, which then allows two conducting plugs, attached to the bar, *v*, to fall into their respective mercury cups, attached, respectively, to the positive and negative binding-posts by a direct wire. This action practically cuts the lamp out of the circuit.

PHILADELPHIA, *Sept.* 19*th*, 1878.

ZIMMERMANN'S STEAM-ENGINE.

By J. H. Cooper.

One of the useful novelties of the present Exposition Universelle, is a horizontal steam-engine, with variable cut-off, controlled by a ball governor and provided with a condensing apparatus on Zimmermann's system, and constructed and exhibited by Messrs. Lecointe and Villette, Saint Quentin, Aisne. From advanced information we give description, illustration and test.

This engine has four double-seated valves, two arranged for foreclosing the admission of steam, and two held open during most of the stroke, by the working gear, for the exhaust; all are placed close to the ends of the cylinder, two above and two below. The admission-valves are controlled by a style of cut-off totally new and perfect, the invention of Mr. Zimmermann, engineer at Marchiennes, Belgium.

This engine offers the following advantages:

Great simplicity of parts, not liable to derangement.

The valves and their seats are of cast steel, the bridges of tempered steel, all of which last a very long time, the wearing of these parts being nearly nothing.

The valves and their seats are independent of, and detachable from, the cylinder casting.

The cylinder is provided with a steam-jacket, and is further protected from cooling by a wooden covering.

The governor is very sensitive, acting promptly on the valve-gear during the run of the engine.

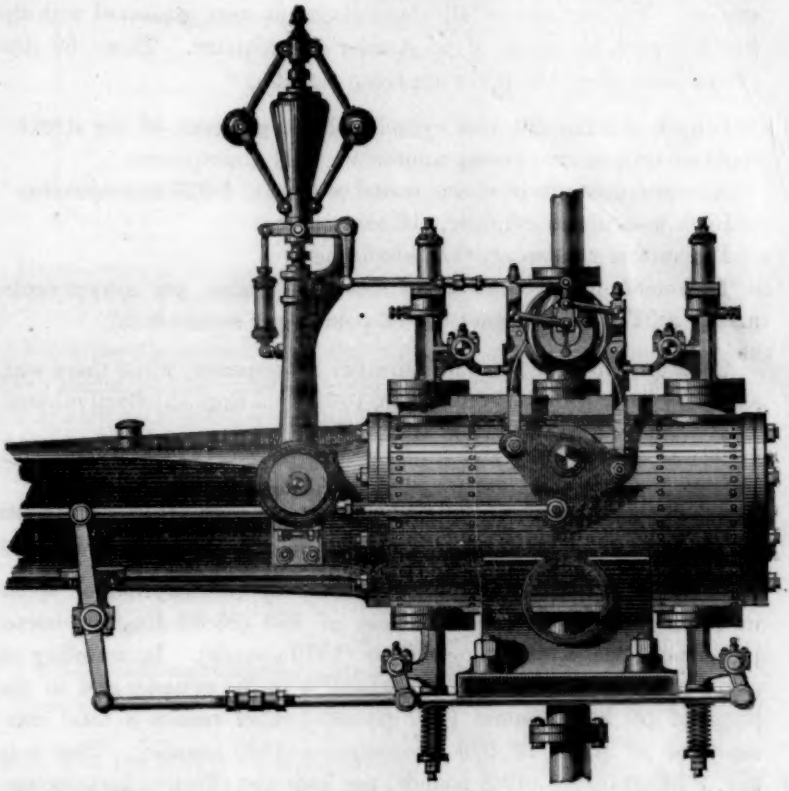
The cylinder and valves are lubricated through one cock, in the usual way.

This engine recommends itself for solidity, elegance, ease of maintenance, regularity of motion, and great economy of fuel.

Its construction throughout is of the best, and the material employed of the first quality.

The following are the results of trials made by a Prony brake and the indicator, on June 29th and 30th, 1877, by Mr. L. Pinrger, Professor in the Polytechnic School at Aix-la-Chapelle, on a steam-engine of 100 (98·63 English) effective horse-power, at work in the cloth factory of Mr. T. F. Mayer.

The engine has valves with automatic variable trip, controlled by a governor on Zimmermann's system. The effective pressure of the steam was 5 kilogrammes (11 pounds), with an absolute pressure of 6 atmospheres; the fly-wheel shaft making 51 revolutions per minute. The cylinder is furnished with a steam-jacket. The stroke of pis-



ton is 1·05 metres (41·34 inches); consequently, the mean speed of the piston per second is 1·785 metres (5·85 feet). The cylinder is ·5 metre (19·685 inches) internal diameter. The piston-rod passes through both cylinder-heads—its diameter is 85 millimetres (3·346 inches). The useful surface of piston is then 1906·75 square centimetres (295·546 square inches).

By means of a Prony brake, acting on a pulley of ·7 metre (27·559 inches) diameter, and keyed on the fly-wheel shaft, there was pro-

duced, in tightening the bolts of the brake, a useful work of exactly 100 (98·63 English) effective horse-power.

During this time a sufficiently great number of diagrams were taken by means of a Richard's indicator, at the two ends of the cylinder. 30 of these diagrams, 15 of which were taken at the front, and 15 at the back end, were used to determine the indicated power of the engine. The surfaces of all these diagrams were measured with the greatest care, by means of an Amsler's planimeter. These 30 diagrams have given the following mean results :

Length of admission into cylinder, 19·86 per cent. of the stroke.
Absolute pressure during admission, 5·76 atmospheres.
Absolute pressure of steam at end of stroke, 1·025 atmospheres.
Back pressure in cylinder, ·16 atmosphere.
Pressure in condenser, ·118 atmosphere.
The mean useful pressure of steam on piston, per square centimetre, 2·5175 kilogrammes (35·72 pounds per square inch).

Consequently, the force in indicated horse-power, while there was, shown by means of the Prony brake, 100 (98·63 English) effective horse-power, is $= \frac{2{\cdot}5175 \times 1906{\cdot}75 \times 1{\cdot}785}{75} = 114{\cdot}3$ (112·73 English), and the useful effect of the engine is $= \frac{100}{114{\cdot}3} = 0{\cdot}875$, or $87\frac{1}{2}$ per cent.

In taking account of the waste spaces, the consumption of steam in the cylinder, for a useful effect of 100 (98·63 English) horse-power per hour, is 850 kilogrammes (1870 pounds). In counting on a loss of steam coming from condensation in the cylinder and in the ports, of 50 kilogrammes (110 pounds), there results a total consumption of steam of 900 kilogrammes (1980 pounds). That is to say, 9 kilogrammes (19·8 pounds) per hour per effective horse-power.

In admitting that 1 kilogramme (2·2 pounds) of coal produces 8 kilogrammes (17·6 pounds) of steam, the consumption of coal per hour per effective horse-power, would be 1·125 kilogrammes (2·475 pounds), or ·985 kilogramme (2·167 pounds) per indicated horse-power.

The temperature of the injection-water in the condenser is 16·5° Cent. (61·7 Fahr.), the temperature of the discharge-water of the condenser 37·5° Cent. (99·5 Fahr). The quantity of water brought

to the condenser $= \frac{600}{21} = 28{\cdot}57$ kilogrammes (62·85 pounds). That is to say, the condensation of 1· kilogramme (2·2 pounds) of steam absorbed 28·57 kilogrammes (62·85 pounds) of water.

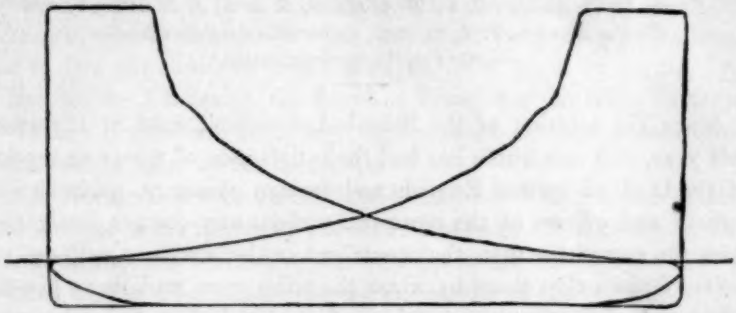


Diagram taken on the engine developing 100 (98·63 English) horse-power, effective under the action of the Prony brake.

From the diagrams taken when doing no work, it results that the mean useful pressure of the steam on the piston is ·165 kilogramme per square centimetre, and the work absorbed

$$= \frac{0{\cdot}165 \times 1906\,75 \times 1{\cdot}785}{75} = 7{\cdot}5 \text{ horse-power (7·19 English).}$$

The work of the steam in the cylinder being 114·3 indicated horse-power to produce a useful work of 100 horse-power on the fly-wheel

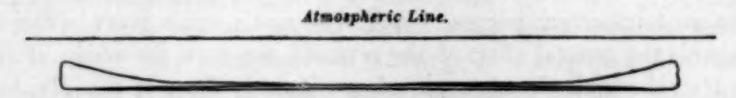


Diagram taken on the engine running empty.

shaft, and the empty machine absorbing 7·5 horse-power, it follows that the work due to the increase of friction, when the machine develops 100 effective horse-power, is only in the neighborhood of 7 horse-power.

This value, which should be considered as exceedingly favorable, comes from the system of distribution, by which the friction is rendered insignificant.

From what precedes, and on account of its working with perfect regularity, the construction and the performance of this machine ought to be classed as *excellent.*

STEERING OF SCREW STEAMERS.

Report of the Committee of the British Association, consisting of James R. Napier, F. R. S., Sir W. Thomson, F. R. S., W. R. Froude, F. R. S., J. T. Bottomley, and Osborne Reynolds, F. R. S., Secr., appointed to investigate the effect of propellers on the steering of vessels.[1]

Since the meeting of the British Association, held in Plymouth last year, this committee has had the satisfaction of receiving reports of the trials of various English and foreign steamers, made by the owners and officers of the steamers, without any further instigation from the committee than that contained in the circulars. These reports all show that those by whom the trials were made have become convinced of the importance of the facts which they have observed. And, indeed, the mere fact of the trials having been undertaken shows that the importance of the effect of the reversed screw on the steering while the ship is stopping herself, is beginning to be recognized. This is further shown by the fact that one of the trials was undertaken at the instance of the Court of Mr. Stipendiary Yorke, in order to ascertain if the captain of the s. s. *Tabor* had been justified in starboarding his helm in order to bring his vessel round to starboard after his screw was reversed.

All these trials, without a single exception, confirm the results obtained in the previous trials made by the committee. But this is not the most important purpose which this year's trials serve. For as regards the general effect of the reversed screw on the action of the rudder, the trials already reported, particularly those of the *Hankow* (see last year's report), are conclusive and leave nothing to be desired. But previous trials were all made with fast vessels at their full draught, their screws being well covered, and the conditions of the weather being most favorable. The trials this year, on the other hand, appear for the most part to have been made with vessels in light trim, and in two instances the wind was blowing with considerable force. The result of these circumstances on the behavior of the vessels is very decided and coincides remarkably with the effects deduced by Professor Reynolds from his experiments on models (see Report, 1875, i, p. 145), viz., that when the screw is not deeply

[1] Presented at the Dublin meeting, Aug. 19th, 1878. The *details* of the trials in 1878 are omitted.

immersed and froths the water, it exerts, when reversed, considerable influence to turn the vessel independently of the rudder; the vessel turning starboard or port according as the screw is right or left handed, which effect (and this seems to be the point most generally unknown) nearly disappears when the screw is so deeply immersed that it does not churn air into the water.

Neither the Admiralty, the Board of Trade, nor the Elder Brethren of Trinity House have taken any further notice of these results communicated to them by the committee.

The Marine Board of South Shields has, however, taken considerable interest in the question, has invited captains to make trials, and Mr. J. Gillie, the secretary, was present at the trial of the *Tabor*, ordered by the Court, and reported the results to the committee.

There have been numerous collisions during the year, and in almost all cases the practice of reversing the screw has been adhered to. In many, if not all, instances where this has been done, the evidence goes to show that the vessel in which the screw was reversed did not turn in the direction in which those in charge of her were endeavoring to turn her. In two important cases this fact was fully apparent even to those in charge of the vessel. And in one instance the owners and captain of vessel attributed the failure to steer to its true cause, namely, the reversal of the screw; although in both cases those immediately in charge of the vessels contended that the rudder was not handled according to their directions.

The first case was that of the *Menelaus* and the pilot schooner on the Mersey. The *Menelaus* was in charge of a first-class pilot, and this steamer, in broad daylight, ran into and sank the pilot schooner, which was dropping up the river with the tide. The pilot in charge contended that owing to the wheel chains having got jammed, his orders were not attended to. The jamming of the chains was denied by the owners; and the fact that they subpœnaed the secretary of the committee to give evidence at the trial, may be taken to indicate the cause to which they attributed the collision.

The case, however, was only in part heard, for after the evidence for the plaintiffs a compromise was effected, and pilots withdrew all assertion that the wheel chains had been jammed, thus admitting that the failure to steer had been brought about by the reversal of the screw.

The other case is the well-known accident to the *Kurfuerst*. In this case it is admitted that the order was to starboard the helm and

reverse the screw of the *Kœnig Wilhelm*, and this order was avowedly given with the view of bringing the vessel round to port. All the experiments of this committee, however, go to prove that with a reversed screw and a starboard helm, such a vessel as the *Kœnig Wilhelm* would have turned to starboard rather than port. This was what, according to all the evidence, did actually happen, and was the final cause of the catastrophe. But it appears that those in charge of the *Kœnig Wilhelm* arrived at the conclusion that the men at the wheel (and these would be many), although they all aver that they heard the order and obeyed it, in reality turned the wheel the wrong way. Considering, therefore, that it was not one man but a number of men at the wheel, and that the vessel behaved exactly as she would have behaved had the order been obeyed, as the men say it was, the conclusion of the Court seems to be most improbable, and for the sake of future steering most unfortunate.

The committee are now of opinion that the work for which they were originally brought together has been fully accomplished. The importance of the effect of the reversed screw on the action of the rudder has been fully established, as well as the nature of its effect completely ascertained. Also, for two years, the committee have urged the results of their work upon the attention of the Admiralty and the various marine boards, and although they regret that, as yet, they have failed to obtain that general recognition of the facts brought to light, which their vital importance demands, they consider that this will surely follow, and that as a committee they can do no more than publish the reports of the trials and the conclusions to which they have been led.

Full accounts of the experiments made previously to this year have been given in the two previous reports, and those which the committee have received this year are given at length at the end of this report. The following is a summary of the conclusions which have been established, and it is interesting to notice that the conclusions drawn by Professor Reynolds, from experiments on models, have been fully confirmed by the experiments on full-sized ships.

Summary of the results of the trials of the effect of the reversed screw on the steering during the time a vessel is stopping herself.

It appears, both from the experiments made by the committee and from other evidence, that the distance required by a screw steamer

to bring herself to rest from full speed by the reversal of her screw is independent, or nearly so, of the power of the engines, but depends on the size and build of the ship, and generally lies between four and six times the ship's length. It is to be borne in mind that it is to the behavior of the ship during this interval that the following remarks apply:

The main point the committee have had in view has been to ascertain how far the reversing of the screw, in order to stop a ship, did or did not interfere with the action of the rudder during the interval of stopping, and it is as regards this point that the most important light has been thrown on the question of handling ships. It is found an invariable rule that, during the interval in which a ship is stopping herself by the reversal of her screw, the rudder produces none of its usual effects to turn the ship, but that, under these circumstances, the effect of the rudder, such as it is, is to turn the ship in the opposite direction from that in which she would turn if the screw were going ahead. The magnitude of this reverse effect of the rudder is always feeble, and is different for different ships, and even for the same ship under different conditions of loading.

It also appears from the trials that owing to the feeble influence of the rudder over the ship during the interval in which she is stopping, she is at the mercy of any other influences that may act upon her. Thus the wind, which always exerts an influence to turn the stem (or forward end) of the ship into the wind, but which influence is usually well under the control of the rudder, may when the screw is reversed become paramount and cause the ship to turn in a direction the very opposite of that which is desired. Also, the reversed screw will exercise an influence, which increases as the ship's way is diminished, to turn the ship to starboard or port according as it is right or left handed; this being particularly the case when the ships are in light draught.

These several influences, the reversed effect of the rudder, the effect of the wind, and the action of the screw, will determine the course the ship takes during the interval of stopping. They may balance, in which case the ship will go straight on, or any one of the three may predominate, and determine the course of the ship.

The utmost effect of these influences when they all act in conjunction, as when the screw is right handed, the helm starboarded, and the wind on the starboard side, is small as compared with the influence

of the rudder as it acts when the ship is steaming ahead. In no instance has a ship tried by the committee been able to turn with the screw reversed on a circle of less than double the radius of that on which she would turn when steaming ahead. So that even if those in charge could govern the direction in which the ship will turn while stopping, she turns but slowly, whereas, in point of fact, those in charge have little or no control over this direction, and, unless they are exceptionally well acquainted with their ship, they will be unable even to predict the direction.

It is easy to see, therefore, that if on approaching danger the screw be reversed, all idea of turning the ship out of the way of the danger must be abandoned. She may turn a little, and those in charge may know in what direction she will turn, or may even, by using the rudder in an adverse manner, be able to influence this direction, but the amount of turning must be small and the direction very uncertain. The question, therefore, as to the advisability of reversing the screw is simply a question as to whether the danger may be better avoided by stopping or by turning. A ship cannot do both with any certainty.

Which of these two courses is the better to follow must depend on the particular circumstances of each particular case; but the following considerations would appear to show that when the helm is under sufficient command there can seldom be any doubt.

A screw steamship when at full speed requires five lengths, more or less, in which to stop herself; whereas, by using her rudder, and steaming on at full speed ahead, she should be able to turn herself through a quadrant without having advanced five lengths in her original direction. That is to say, a ship can turn a circle of not greater radius than four lengths, more or less (see *Hankow*, *Valetta*, *Barge*),[1] so that if running at full speed directly on to a straight coast, she should be able to save herself by steaming on ahead and using her rudder after she is too near to save herself by stopping; and any obliquity in the direction of approach or any limit to the breadth of the object ahead is all to the advantage of turning, but not at all to the advantage of stopping.

There is one consideration, however, with regard to the question of stopping or turning, which must, according to the present custom, often have weight, although there can be but one opinion as to the

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B. A. Reports, 1876—1877.

viciousness of this custom. This consideration is the utter inability of the officers in charge to make any rapid use of their rudder so long as their engines are kept on ahead. It is no uncommon thing for the largest ships to be steered by as few as two men. And the mere fact of the wheel being so arranged that two men have command of the rudder, renders so many turns of the wheel necessary to bring the rudder over that even where ready help is at hand it takes a long time to turn the wheel round and round so as to put a large angle on the rudder.

The result is, that it is often one or two minutes after the order is heard before there is any large angle on the rudder, and of course, under these circumstances, it is absurd to talk of making use of the turning qualities of a ship in case of emergency. The power available to turn the rudder should be proportional to the tonnage of the vessel, and there is no mechanical reason why the rudder of the largest vessel should not be brought hard over in less than 15 seconds from the time the order is given. Had those in charge of steamships efficient control over their rudders, it is probable that much less would be heard of the reversing of the engines in cases of imminent danger.

ON FURTHER APPLICATIONS OF THE FLOW OF SOLIDS.[1]

By M. Henri Tresca, President of the Societé des Ingénieurs Civiles, Paris.

The author has been requested by the Institution of Mechanical Engineers to communicate a second paper on the results of his experiments on the flow of solid bodies, on which subject he read his first paper on the 11th September, 1867.

Prevented by the pressure of other affairs, he has not been able to bestow upon such researches all the attention which they merit; but hopes to be able to respond to the demand by giving expression to some useful conclusions which flow from those already formed from the first experiments.

These original facts can be clearly reconciled and harmonized with our general knowledge of the strength of materials; and more par-

[1] Paper read before the Institution of Mechanical Engineers; Paris meeting.

ticularly with the different phases which are habitually recognized in the phenomena of compression or of extension. This connection, in some degree theoretical, has enabled the author to carry out his new investigations under entirely different circumstances, and to supply fresh evidence in their favor drawn from their character of generality.

For all bodies two distinct periods are recognized—the period of perfect elasticity, which corresponds to variations of length proportional to the pressures applied; and the period of imperfect elasticity, during which the changes of dimensions, on the contrary, increase more rapidly than the pressures. If the second phase of deformation be alone considered, it is easily understood that it leads finally towards a condition in which a given force, sufficiently great, would continue to produce deformation, so to say, without limit—such as may be observed in the process of drawing lead-wire. This particular condition, in which the deformation is indefinitely augmented under the operation of this great force, constitutes, in fact, the geometrical definition of a third period, which has been designated by the author as the period of fluidity, and to which the greater part of his experiments on the flow of solids are related.

The period of fluidity is more extended for plastic substances; it is, necessarily, more restricted, and may altogether disappear in the case of vitreous or brittle substances. But it is perfectly developed in the case of the clays and in that of the more malleable metals.

In his paper of 1867, the author considered the deformations of these substances by flow under certain given conditions; such as the flow of a cylindrical block through a concentric orifice, or through a lateral orifice, one of the most novel subjects of his researches; also plate-rolling, forging, and punching. It was there demonstrated that in these different mechanical actions the pressure was gradually transmitted from place to place, with loss from one zone to another, in absolutely the same manner as in the flow of liquids, and with a regularity not less remarkable, but following a much more rapid law of diminution.

The pressure may be very considerable at certain points, whilst it may be nothing at all at other points, and the study of the various modes in which pressures may be transmitted constitutes, in fact, a new branch of investigation, to which M. de Saint-Venant has given the name of plastico-dynamics. It is chiefly in the operations of punching metals that this mode of transmission of pressure has been

manifested, whilst the processes of forging, on their part, have afforded the means of establishing the correlation between those molecular phenomena, and the development of heat which is their direct consequence.

With respect to the formation of the jets of solid matter similar to jets of liquids, one more experiment only will be referred to, of recent date, by which the likeness is completed, and becomes absolutely illusive.

Two half discs of lead, forming portions of a cylinder 4 inches in diameter, were placed in juxtaposition in the compression-press, so as to form a whole disc. Under the pressure of the piston they resolved themselves into a cylindrical jet, identical in appearance with those jets which had previously been obtained, but formed, in reality, of two semi-cylindrical jets in perfect contact. Their surfaces of contact bore especial traces of the successive movement of the different layers, and reproduced the exact representation, in the solid state, of a sheet of water in motion.

Punching.—Regarded as a question of kinematics, the punching of various substances, as wax, clay, plastic metals, supplies instances of absolutely identical deformations. Shortly after the paper of 1867, some nuts which had been manufactured by punching hot, in England, and which were sent to the author by the kindness of Mr. Bramwell, enabled him to remark the same effects, still better developed by the phenomena of the drawing of the fibres, so well manifested in the specimens now lying on the table.

The two punches, which act in opposite directions, enter the block of metal from opposite sides, and the piece which is left between them is diminished in thickness by flowing from the centre towards the circumference, until, when the two punches are moved in the same direction, the piece reduced to a minimum thickness is shorn off and discharged outside.

The phenomena which take place in this metal, softened by heat, are such as would take place in a liquid; and they lead us to expect that the deformations observed in punching lead should be produced similarly in analogous operations on the hardest of metals.

The author had already shown the inflexion and the curving of the fibres by the punching of discs of cold iron, at the works of MM. Cail & Co.; and also the same phenomena in the burrs which were punched

out; but he had not been able, on account of the insufficiency of his apparatus, to obtain, with iron, as much reduction of the height of the burr, as was obtained in his experiments with more plastic substances.

The section of one of these burrs, taken in a vertical plane through the axis, does not admit of any doubt of the deformations produced.

In a special memoir presented to the Academy of Sciences, on the 31st December, 1869, the author endeavored, on the basis of an enlargement of the burr in the zone of fluidity, as it is called, just under the punch, to establish a general formula for the measure of the reduction of the height of the burr, taking into account the whole height of the burr, its diameter, and the diameter of the punch. The height, L, was given by the formula

$$L = R\left(1 + \log \frac{R}{R_1}\right),$$

in which R and R_1 represent, respectively, the radius of the burr, supposed to be cylindrical, and the radius of the punch.

When the punch penetrates, it forces the material to spread laterally, until the moment when the solid unaltered portion below presents a less amount of resistance to shearing than is applied to the continuation of the lateral spread. This argument suffices to show that all burrs of the same section should be of the same height.

By the results of another and supplementary series of experiments, it was established that for all the different materials, subjected to the same action, the results were substantially alike, and corresponded exactly to the dimensions given by the formula.

But, at that time, the author was unable to experiment with blocks of iron sufficiently thick to embrace a range of evidence as to the reduction of the height of the burr, such as had been obtained with other materials; and it is only quite recently that the results of experiments on punching made in America have appeared, and have in a remarkable manner confirmed *a posteriori* the results of his previous investigations.

Several specimens of these punchings, very skilfully prepared by Messrs. Hoopes and Townsend, have been forwarded from the Philadelphia Exhibition, to the author. But the burrs proved a little longer than the lengths as deduced by means of the formula; the

fact being that the blocks which were sent had been planed after the burrs had been punched out, to dress the faces. When the actual unplaned blocks arrived, they satisfactorily confirmed the algebraic formula.

The reduction of height seemed at first incomprehensible; and it can only be explained by the flow of a portion of the material into that of the block. It is to be remarked, too, that the lower face of the burr is convex, and the upper face is concave; with respect to the latter, the punch only crushes the material at the edge, whilst the middle of the face, notwithstanding the forced passage through the block, retains the original tool marks.

The formula is deduced, as has been seen, from certain hypotheses on the mode in which pressures are transmitted; and though it be only a particular case of more general formulæ, cited in the author's memoir on punching, it retained somewhat of an empirical character. Thanks to the researches of M. Boussinescq, in his theoretical essay on the equilibrium of pulverulent masses compared with that of solid masses, it takes its place as a rational formula, and it may therefore be accepted with complete confidence.

In one specimen only of all those which have been prepared by Messrs. Hoopes and Townsend, the pressure exerted by the flow of the metal has burst the block, and, on a close examination of the bottom of the cavity formed by the punch, in consequence of the mode by which the pressure was transmitted, all the features of the results of the explosion of a projectile there may be found.

A few more sketches of punched blocks are added, showing precisely the contortions produced in the lines of junction by the passage of the punch.

It would be unpardonable if, on this occasion, no mention were to be made of the remarkable experiments on iron compressed when cold, the results of which have already been presented at the Vienna Exhibition, and which have, until now, been only received with doubt, and even with incredulity.

Can the quality of iron really be improved by cold-compression?

There is no longer room for doubt as to this, in view of the recent researches of Professor Thurston, and the numerous specimens which are to be found in the collection of Messrs. Hoopes and Townsend, with the actual particulars of the forces under the action of which they were ruptured.

Speaking now only of the experiments with nuts when punched cold, Professor Thurston's tables indicate a considerable augmentation of resistance relatively to nuts of the same dimensions made of the same iron, and punched hot. The trials were made, either by applying to the rod which carried the nut pressure sufficient to strip the thread, or by introducing into the unscrewed nut a conical mandrel sufficiently loaded to split the nut. The augmentation of resistance due to cold punching may be taken at an average of 25 per cent.; and this result can only be explained by supposing that there is some modification of the molecular condition of the surrounding iron, which has been subjected to compression by the flow from the mass of metal driven out by the punch.

Forging.—If it be necessary to justify the expression, flow of solids, in the case of forgings, it is only needful to prove it by the inspection of a collection of specimens of rail scalings, found on the Eastern Railway, near Epernay. Each blow is in some sort represented by the formation of a wave, and drawing-out has taken place in this fashion, by the formation of successive scales for a length of several decimetres. Deformations produced by forging only differ from this mode of displacement of the molecules, in this, that they are produced for a certain purpose, and at a temperature at which the metal becomes comparatively soft.

The object of the author's early discussions on the forging of iron was to show the tendency to parallelism of all the fibres which originate in drawing-out under the hammer, and which are separated from the neighboring fibres by a cementing substance derived from the incorporated cinder, which fills up all the void spaces between the fibres. This matter is frequently of a vitreous nature, very rich in oxide of iron, and when it is not burned off or pulverized at the surface of the piece when in the hands of the smith, it follows all the varieties of form to which the piece is shaped in its several parts. It has been shown, nevertheless, that the deformation may be only superficial when the action of the hammer was mild, whilst the influence of a more powerful blow, such as is practised in industrial operations, may be felt to the core.

An oblong piece of iron may then be compared to a hank of parallel threads, which will interlock with each other when it is attempted to draw them out lengthwise, but which will separate in a much less regular manner when they are drawn in the crosswise direction, at

the risk of throwing into confusion the regularity of the original arrangement; forming knots and voids which must evidently weaken the power of resistance which would be possessed by the piece under other conditions.

This effect is well exemplified by the specimen of a railing bar, in the formation of which a rectangular bar is transformed, in respect of its transverse section, into a number of rectangles and circles regularly distributed, the fibres in the circular parts losing the parallelism which is visible in the rectangular parts. This condition would certainly be critical, were it not that the central part of the enlargements was afterwards to be bored out.

The interposition of the friable silicates between the fibres which are more properly metallic, ought to be seriously taken into consideration in this case as in many others. At present a few of the more characteristic facts may be noticed.

From the fact that iron wire of good quality is capable of supporting, before giving way, loads much greater than ordinary iron, a manufacturer of best scrap iron tried to work it from piles exclusively composed of wire. A longitudinal section of the bars manufactured in this manner, having been oxidized, reveals the filiform structure of the bar much more clearly than any of the specimens of merchant bar-iron. There is exhibited a specimen taken from an old railing at the Conservatoire which broke spontaneously in its place. Having a greater proportion of the silicates in its composition, which had been imperfectly removed in the process of forging, this specimen exactly reproduces an analogous type.

On the contrary, when the best Swedish iron is submitted to the same operation, it gives but the faintest indications of longitudinal striæ, which sometimes can only be produced by taking special pains with that object.

The irons which are the most effectually purged of silicates are the best, but the expulsion of oxides formed during reheating on the surface of bars designed to be fagotted is of great importance.

The variously-colored appearances that may be raised on well-polished sections, either by a deposit of copper, or by the action of an acid, or, better still, by the action of bichloride of mercury, show clearly the arrangement of the fibres, enabling us to trace, through all the deformation of a piece, the molecular displacements which, but for that demonstration, would remain undetermined.

The treatment by a very weak solution of hydrochloric acid, first employed in the Low Countries by M. de Ruth, is so effective, that by inking the surface, indented at the parts of least resistance by the action of the acid, proofs may be taken, in which the direction of the fibres is perfectly distinguishable. By the employment of chloride of mercury, the indentations and the fibres are much more neatly and delicately defined.

Without reverting to the examples given in the first paper by the author, he will now give other instances in illustration of the most ordinary results from the fibrous constitution of the metal.

On the basis of the evidence supplied by the oxidation of polished sections of iron, M. Le Chatelier[1] sought to separate the siliceous matter which envelops the fibres of the metal, by exposing the iron, at a red heat, to a current of chlorine. The iron is volatilized by this process, and leaves a skeleton as the residue, having the form of the original piece, composed of extremely fine filaments, and resembling, more than anything else, the residue left by a match which quietly burns without inflaming, supposing that the ash is prevented from being pulverized.

This siliceous carcass scarcely amounts in weight to a hundredth part of that of the metal, but it was associated with a certain proportion of iron, which completely disappeared in the course of the operation.

It has been stated that these silicates are friable when cold; and it appears that, with the object of diminishing the wear of bearings, the journals of shafts are sometimes hammered, in order to pulverize this interposed foreign matter, and entirely to clear it away from the rubbing surface.

(To be continued.)

Microphonic Anticipations.—Clerk Maxwell looks for the day when physicians in London will examine their patients by auscultation, in any part of the kingdom; and when the entomological society, putting a microphone in a nest of scorpions under the tropics, will be able to compare their music, in the rooms of the society, with that of the nightingale or that of the cat, and thus increase the interest which has been awakened by Wood Mason's discovery of a musical apparatus in those venomous insects.—*Les Mondes.* C.

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[1] *Comptes Rendus de l'Académie des Sciences*, tome 82, p. 1057.

THE VARIOUS METHODS OF DETERMINING THE VELOCITY OF SOUND.[i]

The propagation of sound is a question with many bearings in the province of physics, and the researches of physicists in relation to it, though numerous, have left some points still under discussion. It is useful in the view of further inquiry to be furnished with a historical survey of what has been already done, and this is the object of a recent memoir by Dr. H. Benno-Mechlenburg, published in Berlin (a *résumé* of which to the following effect appears in the May number of the *Journal de Physique*).

The author has adopted the following classification of the methods that have been employed for measuring the velocity of sound:—

I. Methods requiring the measurement of a time and a course traversed.

1. Direct measurement of the velocity; the most ancient measurements of this kind were executed by P. Mersenne in 1657, by the Academicians of Florence in 1660,[ii] by Walker[iii] (in England), in 1698; by Cassini and Huyghens (in France) etc.

2. Method of coincidences, indicated by Bosscha,[iv] and employed by Koenig.[v]

3. Apparatus of Neumann[vi] and Le Roux.[vii]

II. Estimation of the velocity of sound by the number of vibrations and the wave-length of musical sounds.

A. Direct methods:

1. Method of Bernouilli, with sonorous tubes.
2. Method of Chladni,[viii] with rods.
3. Method of Kundt.
4. Methods of Stefan[ix] and Warburg.[x]

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[i] *Nature.*

[ii] Newton, "Philosophia Naturalis Principia Mathematicæ," II., Prop. XLVIII.—L.

[iii] Laplace, "Méchanique Céleste," t. v, livre xii. p. 115.

[iv] Tentamina, "Exper. Academ. del Cimento," 1738, xi, p. 116.

[v] *Philosophical Transactions*, 1698.

[vi] *Pogg. Ann.*, t. xcii, p. 485.

[vii] *Comptes Rendus*, t. lv, p. 609.

[viii] *Pogg. Ann.*, t. cxxviii, p. 307.

[ix] *Comptes Rendus*, t. lv, p. 662.

[x] Chladni's "Acoustics."

B. Methods based on the interference of sonorous waves:

1. Method of Savart.[i]

2. Method of measurement of the wave-length with Quincke's interference tubes.[ii]

3. Method of Zach.[iii]

4. Method of beats.

The way in which the velocity of sound is affected by certain circumstances, especially intensity and pitch, requires further elucidation. Up till recent times it was believed, in accordance with the earlier observations and the theoretical formulæ of Newton and Laplace, that sound is propagated with a uniform velocity in the same medium, the temperature remaining constant: that the velocity of sound in air at zero, *e. g.*, is an invariable quantity. After an observation by Parry related by Sir James Ross, that the sound of a cannon was always heard sooner than the word of command to fire, Schröder van der Kolck was led by theory to a formula giving the velocity of sound in a gas as a function of the relation of the two specific heats and the degree of compression of the medium. This velocity would be greater the more intense and grave the sound, and would diminish with the distance traversed.

Regnault set himself to determine rigorously the ratio of the two specific heats of gases, with a view to deducing the mechanical equivalent of heat. He remarked that Newton and Laplace had assumed, in their formulæ, that the gases were perfect, *i. e.* (1), that they followed Mariotte's law exactly; (2) that their elasticity was not altered by surrounding bodies; and (3) that gas opposes no inertia to the transmission of sound-waves. Accordingly, the propagation of sound was supposed the same whatever the intensity. Regnault's more complete formula indicated that the velocity is greater the greater the intensity of the wave.

Experiment proved that the intensity of the wave diminishes in a tube more rapidly the smaller the section. The wave is weakened by the reaction of the elastic walls of the tube, causing a considerable

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[i] *Sitzungsberichte der Wiener Akademie*, t. lvii, pp. 197 and 708.

[ii] *Pogg. Ann.*, t. cxxxvi, p. 285.

[iii] *Comptes Rendus*, t. vii, p. 1068.

loss of *vis viva;* and the diminution of intensity, according to Regnault's formulæ, should result in diminution of velocity, which diminution must be more rapid the narrower the tube. This was confirmed by experiment.

As regards experiments with the human voice and wind-instruments, the following are the principal observations of Regnault: acute sounds are propagated with much less facility than grave sounds; in very wide pipes, it is necessary to sing with a baritone voice in order to be well heard; the fundamental sound is heard before its harmonics, which succeed in order of pitch, and the timbre is thus altered. The velocity was found independent of pressure, as indicated by the formulæ. Lastly, with different gases, the velocities are inversely proportional to the square roots of the densities.

In connection with the foregoing, it is interesting to compare the results that have been obtained by Kundt.[1] The idea of his method was suggested by Chladni's figures. A tube of glass is used about 2 m. long, containing a certain quantity of lycopodium powder (distributed as regularly as possible), and closed at the two ends. You rub the tube longitudinally, so as to produce a sound. The powder is then seen to accumulate at the nodes of vibration, so that the sonorous waves of the gas are, in a way, rendered visible. The distance from one node to the next being half a wave length, suppose that we have twelve in the tube; the length of the tube vibrating transversely will be the half of a wave-length in the *glass*. Under such conditions, then, the length of half a wave in the glass is sixteen times the length of half a wave in the air. It will follow that the velocity of sound in the glass is sixteen times that in air. Other gases may be used in the tube, and the velocity of sound similarly found in them.

By a simple modification of the apparatus, this method gives the velocity of sound in a large number of solid bodies, and the results agree pretty closely with those found by different methods.

But Kundt's method does not give sufficient precision in respect to the delicate questions investigated by Regnault. The wave-lengths measured never go beyond about 45 mm., making the 0·0001386 part of the course traversed by sound in a second; hence an error of 0·1 mm. made in the measurement of a wave-length would lead to an

[1] *Pogg. Ann.*, t. v, p. 496.

error of $\frac{1}{10}$ m. in the result sought. With this reserve, Kundt's results may be here noted.

1. The length of sonorous waves, and consequently the velocity of sound, diminishes proportionally to the diameter of the tube, when this is less than a quarter of the length of undulation.

2. In narrow tubes a high sound is transmitted more rapidly than a grave one, and the diminution of the velocity of sound is in inverse ratio to the wave-length.

3. The velocity of sound is independent of the pressure in a wide tube, but increases with it in a narrow one.

It will be seen that these latter results are in contradiction with those found by Regnault.

It may be generally affirmed that every influence which tends to increase the *vis viva* of the molecules of the sonorous medium has an accelerating action on the velocity of sound, and every influence tending to diminish the *vis viva* diminishes also the velocity.

The causes affecting the velocity of sound are, it is shown, various.

In an indefinite medium they are:

1. The temperature of the medium; 2. The quantity of foreign substances found in it, *e. g.*, water-vapor; 3. The pitch of the sound; 4. The direction and force of the wind; 5. In solid bodies, the direction of the sound in relation to the molecular structure.

In sonorous tubes:

6. The diameter; 7. The curvature; 8. The rugosity of the interior surface; 9. The thickness of the walls.

The action of the following additional causes is still disputed:

1. The intensity of the sound; 2. The length of course traversed; 3. The substance forming the tubes.

There is complete disagreement between Regnault, Schröder, Kundt, and Seebeck, as regards the influence of the pitch of the sound. Regnault affirmed merely that an acute sound is transmitted more easily, but not more rapidly, than a grave sound; Schröder finds that the velocity diminishes as the acuteness increases; Kundt and Seebeck reach the contrary result. Fresh experiments are required to settle this important question.

ELECTRICAL LAMP WITH CONTINUOUS CIRCUIT, FOR WORKING IN THE OPEN AIR.[1]

By Emile Reynier.

Translated for the Journal of the Franklin Institute, from *La Nature*, by P. Pistor, M. E.

If a very intense current of electricity is led through a resisting and refractory conductor, such as a pencil of carbon, the temperature of this conductor may rise to a dazzling white heat; it then emits a vivid light. This is the well-known principle of electrical lamps.

The principal difficulty to be overcome in the electrical lamp, is to limit the undue waste of the luminous conductors; a waste which is very rapid even in an enclosed space, on account of the volatilization and disaggregation of the carbon pencils; and which is greatly accelerated in the open air, by the rapid combustion of the incandescent carbon.

In the various systems of electrical lamps with continuous conductors heretofore proposed, the renewing of the carbon points is performed in the following manner: The incandescent pencil is placed in the circuit, with fixed contacts, and remains part of the same until broken by being consumed. The light is then extinguished. The current now suddenly passes from this carbon to another, which is consumed, the circuit broken in itsturn, and so on.

This method is open to many objections; there is an interruption of the current accompanied by an extinction of light at every rupture of the pencil. The luminous intensity varies continually on account of the gradual thinning of the carbon. The conductor only gives its maximum of light at the moment next to that of rupture. Finally, the proposed apparatus can scarcely work except in an enclosed space.

In the new system which forms the subject of this communication, the renewal of the carbon is progressive. The carbon, incandescent a part of its length, advances almost continuously, till the whole available part has been consumed. This system can operate in the open air. The following is the principle: A cylindrical or prismat-

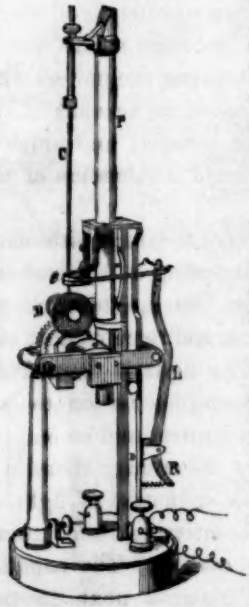
[1] Resume of a communication to the French Physical Society, at the meeting of May 17th, 1878.

ical pencil of carbon, *C*, Fig. 1, between *i* and *j*, forms part of an electrical circuit (continuous or alternate), sufficiently intense to render this part incandescent. The current enters or leaves at the point of contact, *l*, it leaves or enters at the point of contact, *B*. The contact *l*, which is elastic, compresses the pencil laterally; the contact *B* touches it at its end. Under these conditions the carbon is consumed at its extremity sooner than at any other place, which tends to diminish its length. Consequently, if the carbon is steadily forced in the direction of the arrow, it will gradually advance as it is consumed, sliding through the lateral contact *l*, so as to press continuously on the point of contact at *B*. The heat developed by the passage of the current is greatly increased by the combustion of the carbon.

Fig. 1.

Fig. 2.

Fig. 3.

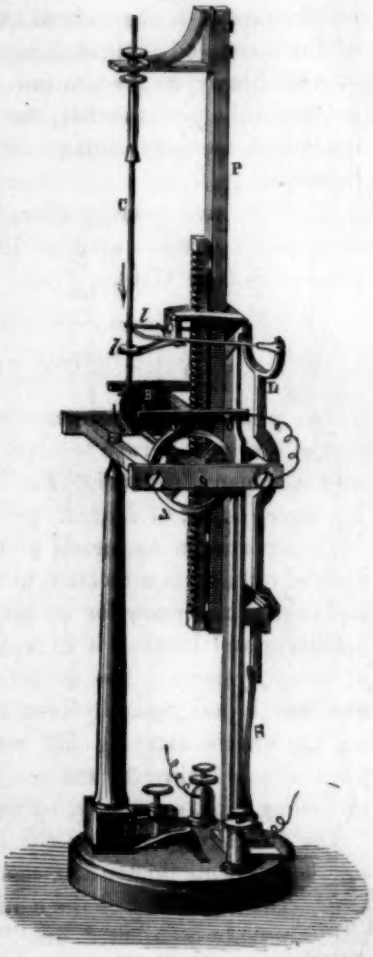


In practice I substitute for the fixed contact a revolving contact *B*, Fig. 2, which carries off the cinders of the carbon. The rotation of the contact is made dependent on the progressive movement of the carbon, so that the weight of the latter exerted at its end, acts as a brake on the mechanism of the motion.

The principle of this new system once established, simple apparatus to realize it could easily be devised. The specimens I submitted to the Society of Physics, may be understood at the first glance. The advance of the carbon *C*, Fig. 3, and revolving contact at *B*, are obtained by means of the descent of the heavy rod *P*. To wind up the lamp, it is only necessary to raise this rod. The carbon pencil is placed in its position without any adjustment. The luminous point remains fixed, which is always advantageous, and particularly so in optical experiments.

This apparatus gives a clear white light with four Bunsen elements. With a more powerful electrical source, several lamps of this system may be operated, and thus "a subdivision of the electrical light may be obtained."

Fig. 4.



I have made the following experiments before the Society of Physics: with a battery of 36 elements of 18 centimetres, grouped in two series of 18 each, I have operated "four" lamps placed in a "single" current. I have at will repeatedly extinguished and re-lighted the four lamps. Each of the four lamps could be extinguished and re-lighted "individually," the three others continuing unaffected. Light has been obtained from one of these lamps by means of the current of a small Gramme Machine for the laboratory with treadle attachment.

Finally, a fine light has been obtained with a battery of three Planté (secondary), elements which were charged during the afternoon, at the establishment of M. Bréguet, and carried, charged, to the hall of the society. I remarked that this experiment may be considered as a step towards the application of the electrical light to "domestic purposes."

I will close by describing a new and unpublished mechanical arrangement of my apparatus.

In this arrangement the revolution of the turning contact is obtained from the tangential component of the pressure of the carbon pencil on the circumference of the disc; thus the end of the pencil never leaves the revolving contact, and all causes of irregularity in the light are obviated.

The brake, always indispensable, is operated, Fig. 4, in the following manner: the disc *B* is carried by a lever swinging from *O;* the pressure exerted by the carbon on the disc *B*, causes the shoe *S* to press on the face of a wheel *A*, which is revolved by means of the weight of the heavy rod *P* through its rack and the pinion *a*.

Accordingly, as the point of the luminous conductor presses more or less heavily on the disc, the brake will retard, more or less, the descent of the heavy column, which occurs at almost inappreciable intervals.

This new arrangement differs from that presented to the Academy of Sciences (session of May 13th), and to the Society of Physics (session of May 17th).

WIRE ROPE TRANSPORTATION.

Der practische Maschinen-Constructeur gives a very full description of a system of rope transportation as constructed by the "Tramway Company, limited," but considerably modified by Franz. Schingen, C. E., of Düren.

Transportation by means of a wire rope as a feeder to the main lines of railway, is calculated to supply a want long felt. The time and capital necessary for its construction, as well as the expense of operating and keeping it in repair, are considerably less than those of other systems. The use of a wire rope for this purpose is not new, but it has generally been used merely as a support and guide for the wheels carrying the load; the necessary power being supplied on an incline by the weight of the load, by an independent propelling rope, or, finally, by hand-power.

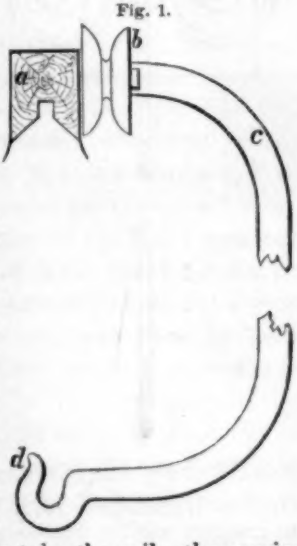
The difficulty of passing the load over the supports has been the chief cause of its failure as a combined means of support and propulsion. The regulating of the motive power was also impossible, as the load, unevenly distributed over the rope, caused a sagging of the same between the support, and, consequently, alternately exerted a force in the same direction, and then against the propelling power.

These difficulties have been overcome by adopting (1) an endless wire rope, moving over supporting rollers; (2), a distribution of the load on several carriers, instead of one, following each other at regular intervals.

It is thus possible to equalize the opposing strains caused by the sagging of the rope on either side of the support, and also to obtain a steady motion. The greater the number of carriers on the rope, and the more even their distribution, the steadier the motion. A complete set of working drawings accompany the article.

To obtain the necessary friction, the rope first passes over the traction pulley, then over a tension pulley, and, finally, over a loose pulley on the shaft of the former. The last two pulleys have flanges and a wooden facing. The tension pulley is held in place by counter-weights attached to a rope passing over rollers. The weights rise and fall, according to the strain on the rope. The whole arrangement rests on a wooden frame, which should be built on a stone foundation, with anchor bolts passing through, on account of the many jars which cannot be avoided, especially when starting the rope. The arrangement for unloading is as follows: A heavy frame carries four pulleys, two on each side, a tension and a guide pulley. The slack side of the rope runs first under the tension, and then over the guide pulley. The tension side first over the guide, then under the tension pulley. Near the centre of the guide pulleys, on both sides of the frame, are two rails of flat iron, on edge, bent so as to incline down and join below the rope, so that the carrier may be run over to either side of the rope. When the wheels take the rails, the carrier leaves the rope, and it is switched off in this manner.

Fig. 1.

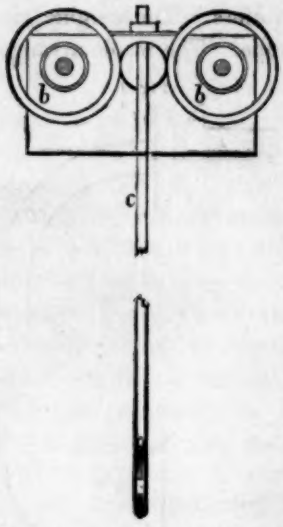


The station for loading is similar to the above. As the rope is to return from this point, it is led over a guide pulley, then over one inclined in such a manner as to lead on to the second guide pulley. The arrangement of the rails for running the carrier off the rope, is the same as before.

The carrier is composed of two blocks of wood, *a*, Figs. 1 and 2, sheathed in iron, linked together, each having a groove its entire length, and two small double-flanged wheels, *b*, on one side. A bent iron rod, *c*, from each block carries the suspended load. The wheels of

the loaded carrier rest on the rails at the station. It is allowed to roll down the incline, the block slides on to the rope, which immediately presses itself so tightly into the closely-fitting groove that, even when passing over the supporting pulleys, it does not leave the groove, whose slanting sides fit closely over the rim of these pulleys. There is, consequently, no danger of the load sliding when traveling up the sag of the rope. Arrived at the station to unload, the wheels take the rails, which reach their highest level at the guide rolls. The rope, immediately after passing over these, parts with the block, the tension pulley carrying it downwards. The empty carrier is run onto the rope, slack side, in the reverse manner to return.

Fig. 2.



The rope is carried around curves by means of vertical and horizontal guide pulleys, the number of which depends on the angle of curvature. It is claimed that a curve of 8 ft. radius, for the inner rope, is practicable, though it is advisable to take a larger radius if possible. In order to pass the carrier around the curve, it is run off the rope in the same manner as at the unloading station, passes around the curve on the inclined rails, and drops on to the rope automatically. Branch roads can be constructed in the same manner, and the carriers transferred from one line to the other by means already described.

It is not advisable to make the supports more than 23 ft. in height. A heavy grade (1 : 6) is not objectionable. The guide pulleys should be carefully lined up. Their diameter depends on the size of the rope, and this, finally, on the load to be transported. P.

Velocity of Steamships.—Since 1840 the Atlantic steamers have increased their speed from 8·3 knots per hour to 15·6 knots, while the supply of fuel per 100 horse-power has been reduced nearly 60 per cent. In 1877, there were 182 steamers in service, with a freight capacity of 556,560 tons.—*Fortsch. d. Zeit.* C.

Work of Diffusion.—Preston thinks that the mechanical labor accomplished in the diffusion of gases is at variance with the second law of thermodynamics, inasmuch as it involves a transfer of heat from a colder to a warmer body. Clausius replies that if that transfer was so made as to restore the variable material at the close of the process to its original relations, the objection would be good. But this is not the case. The two gases at the beginning are unmixed, and at the close they are mixed. During the mixture there is an evident change, accompanied by an expansion of the gases, and it is in consequence of this expansion that the apparent reversal of the law takes place.—*Ann. der Phys. u. Chem.* C.

The Planet Vulcan.—The observation of an intra-Mercurial planet by Profs. Watson and Swift, naturally awakens an interest in the determination of its distance from the sun. The only data upon which an estimate of the distance can be based, are the group of supposed observations which Leverrier used in his calculations of the perturbations of Mercury, and the harmonic prediction which was communicated to the American Association, in 1873, and strikingly confirmed by the sun-spot observations of De la Rue, Stewart and Loewy. Mouchez and Gaillot, in communications to the French Academy, estimate the probable period of revolution at 24·25 days, thus making the orbital time somewhat less than that of solar rotation.—*Comptes Rendus.* C.

Zodiacal Light and Sun-Spots.—Among the most important results of the observations upon the recent solar eclipse, are the evidences of a connection between the brilliancy of the corona and the number of sun-spots, and the indications of a meteoric action within the corona. Cassini observed that the zodiacal light is most brilliant when the sun has large and numerous spots. Olbers, in a letter to Grinthuisen, published in his Astronomisches Jahr Buch for 1840, says that his grandson, Wilhelm Focke, had observed an unusual brilliancy in the zodiacal light in the early part of 1839, when the sun-spots were very numerous. These combined observations seem to lend confirmation to the meteoric theory of the zodiacal light.—*Les Mondes.* C.

Bismuth Bronze.—A bronze, composed of 52 parts copper, 30 parts nickel, 12 parts zinc, 5 parts lead, and 1 part bismuth, resists oxidation, and is especially fitted for the preparation of mirrors, lamp reflectors, etc.—*Fortsch. der Zeit.* C.

Hot Air and Steam in Puddling Furnaces.—Lemut describes an apparatus for combining hot air and superheated steam in puddling furnaces. The grates, the sides of the fire-boxes and of the ash-pit, and all the hottest portions of the apparatus, are connected with air-chambers, which are so supplied with vapor as to increase their durability, and at the same time to furnish an ample quantity of air for the draft, heated to a temperature of from 450° to 500°. By means of this elevated temperature he has been able to introduce superheated steam under the grate, and to realize an important saving by its decomposition. The total saving effected by his apparatus, he estimates at more than 25 per cent.—*Ann. des Mines.* C.

Non-existence of Electric Elongation.—Edlund, in 1866, and Streinitz, in 1873, concluded that a conductor, when traversed by an electric current, was lengthened in consequence of the electric influence. Wiedemann regarded their experiments as inconclusive and the question as an open one. Blondlot, by examining the phenomena both in the lateral and in the longitudinal direction, concludes that the phenomena are due entirely to heat, and that the passage of an electrical current in a metallic conductor produces no mechanical effect of lengthening or of shortening. His apparatus was so sensitive, that a variation of ·00025 mm. would have been sensible.—*Comptes Rendus.* C.

Variations of Terrestrial Magnetism.—M. Quet, starting from the hypothesis of an artificial earth, suitably constructed in regard to the conduction of electricity and magnetism, turning upon its axis and revolving about a sun which diffuses electrical currents, calculates the result which ought to ensue. Applying the general principle of La Place, that the state of a system of bodies becomes periodic when the effect of the primitive conditions of movement has disappeared by the action of resistances, he finds that the diurnal and annual variations, and the opposition in the character of the variations in the northern and southern hemispheres, should all be such as have been actually observed.—*Comptes Rendus.* C.

Early Use of Iron.—The oldest pieces of wrought iron which are known are, probably, the sickles which were found by Belzoni under the pedestal of the sphynx in Karnac, near Thebes; the blades which Wyse found imbedded in the wall of the great pyramid, and the piece of a saw which Layard dug up at Nimroud. These remains are now owned by the British Museum.—*Fortsch. der Zeit.* C.

Combination by Volumes.—Schröder generalizes his investigations upon volume combination, as follows: The volumes of different bodies stand in simple relations; *A*, for gases, as is well known, under equivalent pressures and temperatures. *B*, for fluids, as he first showed in 1843, under equivalent vapor pressures, and as he has subsequently shown, under equivalent solidities of the combined elements. *C*, for solid bodies, under equivalent solidities of the assimilating elements.—*Ann. der Phys. u. Chem.* C.

Origin and Periodicity of Tempests.—Zenger (Mem. de l'Acad. des Sci. de Bohéme) has been examining a table, published by Argelander, of the tempests observed at Helsingford and Abo, between 1821 and 1831. Finding that these tempests often correspond to magnetic storms and auroras, he began, in 1875, a series of solar observations. On examining photographs of the sun, he found that in tempests the disc seemed to be surrounded by an absorbing couch, which was indicated upon the plates under the form of a circle, an ellipse, a parabola, or bands, the breadth of which varies from a semi-diameter to six diameters of the sun. The appearance of this zone sometimes precedes the beginning of a storm by a day, and anticipates any barometric indications. In a journey to Tyrol and Switzerland, observations which were made at altitudes varying from 5,000 to 10,000 feet, gave similar results but still more striking than in the plain. On comparing the dates of the photographs he found that tempests seemed to occur at periods of from ten to thirteen days. Similar intervals were found in tables of American tornadoes from 1794 to 1854, and of Chinese typhoons from 1783 to 1876. The storm charts show, moreover, the existence of two great storm centres; the Island of St. Thomas, in the West Indies, and the coast of Farther India, about latitude 18° to 20°; the two centres being about 180° apart. All of the disturbances seemed to be dependent upon influences which returned at every semi-rotation of the sun.

C.

Ozone.—Eremine finds that in solutions of oxalic acid, ozone may be kept much longer than in any other fluids that have been hitherto tried. Moreover, the decomposition of the ozone, in daylight, is slower than in darkness; the ozone which has been left for some time after its preparation, has a much more energetic disinfecting power.—*Les Mondes.* C.

Mosandrum; a New Element.—Prof. J. Lawrence Smith, on September 22d, 1877, deposited a sealed packet in the French Academy, describing a new earth of the cerium group. He sent specimens of the earth to Delafontaine, Soret, and others, who examined it, some thinking that it might be the terbium of Marignac, while others supposed that it was entirely unknown. The spectroscopic observations of M. Soret demonstrated that it was formed from a new element akin to the Yttria group, to which he has given the name of Mosandrum, in honor of Mosander, the discoverer of terbium.—*Comptes Rendus.* C.

Geological Progress.—Delesse and De Lapparent, in their last geological summary, quote largely from the reports of Prof. Lesley and his assistants on the second geological survey of Pennsylvania. They mention numerous instances of the discovery of native iron in trap rocks and in dolorite, and consider that the immense masses of native iron in Greenland, are telluric instead of meteoric, resulting from a reduction by petroleum or graphite, or by the organic matters which are commonly found in basaltic and trap rocks. The investigations of Viollet Le Duc and Grad lead to the conclusion that glacial action, so far from having hollowed out valleys, has protected them from erosion. Milne thinks that the action of icebergs has been generally exaggerated, and that the depth of the submerged portion of an iceberg is rarely so great as the height of the pyramidal portion which is out of water. De la Grye has proved, by experiments made at Rochelle, that the quantity of mud at the bottom of a stream is often ten times as great as that at its surface. He has frequently found a true river of mud rolling at the bottom of the sea. Buchanan and Murray have shown that the supposed organism, bathybius, is nothing but a gelatinous precipitate of calcium-sulphate, and that there is no protoplasm in sea-water which furnishes any appreciable quantity of organic matter, except foraminiferæ and algæ.—*Ann. des Mines.* C.

Heating Wheel Tires.—In the workshops of the railway from Moscow to Nijni, the wheel-tires are plunged, by the help of a movable crane, into a metallic reservoir filled with water, which is kept boiling by the admission of steam from a neighboring boiler. After ten minutes immersion, the tire is put upon the wheel. The heat is more uniform than that which is given by fire, and the contraction is, consequently, more regular. In six years the water-treated tires only furnished one fracture, and less than one per cent. have become loosened upon the wheel.—*Ann. des Ponts et Chauss.* C.

Telephone without a Diaphragm.—Mr. F. G. Lloyd, in *Nature*, gives the following account of a very interesting experiment: Two ordinary electro-magnets—unscrewed from a couple of large electric bells—were fastened, by means of two little wooden saddles and a screw each, to a small piece of deal board about 4½ inches square and ⅝ inch thick, in such a way that the poles were all but touching. Their wires were then joined so that poles of opposite denominations faced each other, *i. e.*, north opposite south, and *vice versa*. This, placed on an empty cigar-box and four Leclanché cells in circuit, gave out the tune of a musical-box clearly and loudly in the room. When both poles were made to touch, the sound ceased; but with a thin piece of paper or stout tinfoil between them, without any intervening air space, the sound was heard. On gradually separating the magnets, the sounds grew fainter and fainter, till they became inaudible. By putting the bass-board close to the ear, whistling and singing to the microphone were very clearly and loudly heard, also the voice of the person speaking could be recognized; but words were hardly sufficiently defined to distinguish all that was said, though now and then parts were intelligible. One of the electro-magnets was afterwards replaced by a small permanent steel horseshoe-magnet fastened to the board in a similar manner; the result was the same, but slightly louder, probably from there being less resistance. By varying the strength of battery, size, or mode of mounting magnets, or adjustment of the microphone, I have no doubt that perfect definition can be obtained. Thus it would appear that the electro-magnet, without any diaphragm whatever, can be made a reproducer of sounds transmitted by a Hughes' microphone, and thus a complete and practical telephone system produced without the possibility of infringing anybody's patent. *

PREDICTION CONFIRMED.

HAVERFORD COLLEGE, *Sept. 9th*, 1878.

Editors of the JOURNAL OF THE FRANKLIN INSTITUTE.

GENTLEMEN:—Gaillot's estimated orbit for Watson's inner planet (*Comptes Rendus*, 5 Août 1878), accords closely with the predictions which I made in 1873 (*Proc. Am. Phil. Soc.*, xiii, 238, 472). It represents the third of my harmonically-indicated intra-Mercurial planets:

	Distance.	Time.
Gaillot (computed), . . .	·164	24·25 dys.
Chase (predicted), . . .	·165	24·50 "

—Yours truly,

PLINY E. CHASE.

Book Notice.

FRICK'S PHYSICAL TECHNICS; a Manual for Experiments in Physics. J. B. Lippincott & Co., Phila., 1878.

This work, which first appeared in this country in 1861, has met with deserved commendation, as being a very useful book. Its scope was amply large enough to include all the experiments needed to illustrate a very full course of lectures on physics, and the detailed descriptions of materials used, and of methods of construction of apparatus, gave it great value. A book of this kind is especially needed by the many teachers of physics, who are out of the reach of skilled mechanics, or who have not the means at their disposal to enable them to purchase fine physical apparatus.

We are sorry, however, that the work has not been revised and brought down to date. What we thought, from the title-page, to be a revised edition, is simply a reprint, verbatim, of the edition of 1861, and as the preface to that was dated 1856, the book represents the science of physics as it was 22 years ago. As this was before the discovery of spectrum-analysis, we find no mention of the spectroscope whatever in its pages. Nor are there any of the beautiful experiments of Helmholtz and König in acoustics. Nor do we find a word concerning the Holtz electrical machine, or similar modern forms of apparatus. We feel certain that no course of experimental lectures on physics, at the present day, leaves these important topics untouched. S.

Franklin Institute.

HALL OF THE INSTITUTE, Sept. 18th, 1878.

The stated meeting was called to order at 8 o'clock P. M., the Vice-President, J. E. Mitchell, in the chair.

There were present 145 members and 58 visitors.

The minutes of the last meeting (June 19th) were read and approved.

The Actuary presented the minutes of the Board of Managers, and reported that at the last meeting the following donations were made to the Library:

Illustrated catalogue of well excavators and prospecting machinery. From C. D. Pierce.

Transactions and Proceedings of the New Zealand Institute, vol. 10. 1877. By Jas. Hector. From the Institute.

Pennsylvania Archives. Second series. Vol. 7. Harrisburg, 1878. From J. Blair Linn.

Year Book of Education for 1878. From E. Steiger, New York.

Southall, J. C. Address on Man's Age in the World. From Board of Visitors of University of Virginia.

Specifications and drawings of patents issued from the United States Patent Office, for February, 1878. Washington. From the Patent Office.

Register of the Commissioned, Warrant and Volunteer Officers of the Navy of the United States, etc., to July 1st, 1878. Washington. From Secretary of the Navy.

Simmonds, P. L. Waste products and undeveloped substances. From S. Needles.

Map of British Columbia, showing coal-fields, etc., to illustrate report of Mr. J. Richardson, of the Geological Survey of Canada. 1876–7. From the Survey Office.

Report of the Superintendent of the United States Coast Survey, for 1875. Washington, 1878. From the Survey.

Annals of the Astronomical Observatory of Harvard College. Vol. 4, Part 2, and Vol. 9. From the College.